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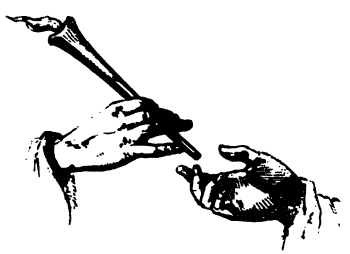
FROM THE EARLIEST TO THE PRESENT TIMES.

BY THE

REV. WILLIAM WHEWELL, M.A.,

FELLOW AND TUTOR OF TRINITY COLLEGE, CAMBRIDGE; PRESIDENT OF THE GEOLOGICAL SOCIETY OF LONDON.

IN THREE VOLUMES.



Λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις.

VOLUME THE SECOND.

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ADDITIONAL CORRECTION IN VOL. II.

Page 105, line 14. Euler was the true author of the method of the *variation of elements*. The first essay of this method appears in a memoir in 1749; and it was further developed in another memoir in 1756, ten years before that of Lagrange mentioned in the text. See Laplace, Mécanique Céleste, livre xv., page 305, 310.

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HISTORY

OF

THE INDUCTIVE SCIENCES,

&c.

VOLUME THE SECOND.

BOOK VI.

THE MECHANICAL SCIENCES.

HISTORY OF MECHANICS,

INCLUDING

FLUID MECHANICS.

As pilot well expert in perilous wave
That to a steadfast star his course hath bent,
When foggy mists or cloudy tempests have
The faithful light of that fair lamp yblent,
And covered heaven with hideous dreriment;
Upon his card and compas firms his eye,
The maysters of his long experiment,
And to them does the steddy helm apply,
Bidding his winged vessel fairly forward fly.

SPENSER, *Faerie Queen*, b. ii. c. 7.

INTRODUCTION.

WE enter now upon a new region of the human mind. In passing from Astronomy to Mechanics we make a transition from the *formal* to the *physical* sciences;—from time and space to force and matter;—from *phenomena* to *causes*. Hitherto we have been concerned only with the paths and orbits, the periods and cycles, the angles and distances, of the objects to which our sciences applied; namely, the heavenly bodies. How these motions are produced;—by what agencies, impulses, powers, they are determined to be what they are;—of what nature are the objects themselves;—are speculations which we have hitherto not dwelt upon. The history of such speculations now comes before us; but, in the first place, we must consider the history of speculations concerning motion in general, terrestrial as well as celestial. We must first attend to Mechanics, and afterwards return to Physical Astronomy.

In the same way in which the developement of pure mathematics, which began with the Greeks, was a necessary condition of the progress of formal astronomy, the creation of the science of mechanics now became necessary to the formation and progress of physical astronomy. Geometry and mechanics

were cultivated for their own sakes; but they supplied ideas, language, and reasoning to other sciences. If the Greeks had not cultivated Conic Sections, Kepler could not have superseded Ptolemy; if the Greeks had cultivated Dynamics, Kepler might have anticipated Newton.

INTRODUCTION.

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in such a manner as to show that his followers had not clearly apprehended the idea on which his reasoning altogether depended. The clouds which he had, for a moment, cloven in his advance, closed after him, and the former dimness and confusion settled again on the land.

This dimness and confusion, with respect to all subjects of mechanical reasoning, prevailed still, at the period we now have to consider; namely, the period of the first promulgation of the Copernican opinions. This is so important a point that I must illustrate it further.

Certain general notions of the connexion of cause and effect in motion, prevail at all periods of the developement of the human mind, and are implied in the formation of language and in the most familiar employments of men's thoughts. But these do not constitute a *science* of mechanics, any more than the notions of square and round make a geometry, or the notions of months and years make an astronomy. The unfolding these notions into distinct ideas, on which can be founded principles and reasonings, is further requisite, in order to produce a science; and, with respect to the doctrines of motion, this was long in coming to pass: men's thoughts remained long entangled in their primitive and unscientific confusion.

We may mention one or two features of this confusion, such as we find in authors belonging to the period now under review.

We have already, in speaking of the Greek school

philosophy, noticed the attempt to explain some of the differences among motions, by classifying them into natural motions and violent motions; and the assertion that heavy bodies fall quicker in proportion to their greater weight. These doctrines were still retained: yet the views which they implied were essentially erroneous and unsound; for they did not refer distinctly to a measurable force as the cause of all motion or change of motion; and they confounded the causes which produce, and those which preserve, motion. Hence the study of such principles did not lead immediately to any advance of knowledge, though efforts were made to apply them, in the cases both of terrestrial mechanics and of the motions of the heavenly bodies.

The effect of the inclined plane was one of the first, as it was one of the most important, propositions, on which modern writers employed themselves. It was found that a body, when supported on a sloping surface, might be sustained or raised by a force or exertion which would not have been able to sustain or raise it without such support. And hence, *The Inclined Plane* was placed in the list of Mechanical Powers, or simple machines by which the efficacy of forces is increased: the question was, in what proportion this increase of efficiency takes place. It is easily seen that the force requisite to sustain a body is smaller, as the slope on which it rests is smaller; Cardan (whose work, *De Proportionibus Numerorum, Motuum, Ponderum,*

&c., was published in 1545) asserts that the force is double when the angle of inclination is double, and so on for other proportions; this is probably a guess, and is an erroneous one. Guido Ubaldi, of Marchmont, published at Pesaro, in 1577, a work which he called *Mechanicorum Liber*, in which he endeavours to prove that an acute wedge will produce a greater mechanical effect than an obtuse one, without determining in what proportion. There is, he observes, "a certain repugnance" between the direction in which the side of the wedge tends to move the obstacle, and the direction in which it really does move. Thus the wedge and the inclined plane are connected in principle. He also refers the screw to the inclined plane and the wedge, in a manner which shows a just apprehension of the question. Benedetti (1585) treats the wedge in a different manner; not exact, but still showing some powers of thought on mechanical subjects. Michael Varro, whose *Tractatus de Motu* was published at Geneva in 1584, deduces the wedge from the composition of hypothetical motions, in a way which may appear to some persons an anticipation of the doctrine of the composition of forces.

There is another work on subjects of this kind, of which several editions were published in the sixteenth century, and which treats this matter in nearly the same way as Varro, and in favour of which a claim has been made[1] (I think an unfounded one,) as if it

[1] Mr. Drinkwater's Life of Galileo, in the Lib. Usef. Kn. p. 83.

contained the true principle of this problem. The work is "Jordanus Nemorarius *De Ponderositate*." The date and history of this author were probably even then unknown; for in 1599, Benedetti, correcting some of the errors of Tartalea, says they are taken "a Jordano quodam antiquo." The book was probably a kind of school-book, and much used; for an edition printed at Frankfort, in 1533, is stated to be "Cum gratia et privilegio Imperiali, Petro Apiano mathematico Ingolstadiano ad xxx annos concesso." But this edition does not contain the inclined plane. Though those who compiled the work assert in words something like the inverse proportion of weights and their velocities, they had not learnt at that time how to apply this maxim to the inclined plane; nor were they even able to render a sound reason for it. In the edition of Venice, 1565, however, such an application is attempted. The reasonings are founded on the usual Aristotelian assumption, "that bodies descend more quickly in proportion as they are heavier." To this principle are added some others; as, that "a body is heavier in proportion as it descends more directly to the centre," and that, in proportion as a body descends more obliquely, the intercepted part of the direct descent is smaller. By means of these principles, the "descending force" of bodies, on inclined planes, was compared, by a process, which, so far as it forms a line of proof at all, is a somewhat curious example of confused and vicious reasoning. When two bodies are supported

on two inclined planes, and are connected by a string passing over the junction of the planes, so that when one descends the other ascends, they must move through equal spaces on the planes; but on the plane which is more oblique (that is, more nearly horizontal,) the vertical descent will be smaller in the same proportion in which the plane is longer. Hence, by the Aristotelian principle, the weight of the body on the longer plane is less; and, to produce an equality of effect, the body must be greater in the same proportion. We may observe that the Aristotelian principle is not only false, but is here misapplied; for its genuine meaning is, that when bodies *fall freely* by gravity, they move quicker in proportion as they are heavier; but the rule is here applied to the motions which bodies *would* have, if they were moved by a force extraneous to their gravity. The proposition was supposed by the Aristotelians to be true of *actual* velocities; it is applied by Jordanus to *virtual* velocities. This confusion being made, the result is got at by taking for granted that bodies *thus* proved to be equally *heavy*, have equal powers of descent on the inclined planes; whereas, in the previous part of the reasoning, the weight was supposed to be proportional to the descent in the vertical direction. It is obvious, in all this, that though the author had adopted the false Aristotelian principle, he had not settled in his own mind whether the motions of which it spoke were actual or virtual motions;—motions in the direction of the inclined

plane, or of the intercepted parts of the vertical, corresponding to these; nor whether the "descending force" of a body was something different from its weight. We cannot doubt that, if he had been required to point out, with any exactness, the cases to which his reasoning applied, he would have been unable to do so; not possessing any of those clear fundamental ideas of pressure and force, on which alone any real knowledge on such subjects must depend. The whole of Jordanus's reasoning is an example of the confusion of thought of his period, and nothing more. It no more supplied the want of some man of genius, who should give the subject a real scientific foundation, than Aristotle's knowledge of the proportion of the weights on the lever superseded the necessity of Archimedes's proof of it.

We are not, therefore, to wonder that, though this pretended theorem was copied by other writers, as by Tartalea, in his *Quesiti et Inventioni Diversi*, published in 1554, no progress was made in the real solution of any one mechanical problem by means of it. Guido Ubaldi, who, in 1577, writes in such a manner as to show that he had taken a good hold of his subject for his time, refers to Pappus's solution of the problem of the inclined plane, but makes no mention of that of Jordanus and Tartalea. No progress was likely to occur, till the mathematicians had distinctly recovered the genuine idea of pressure, as a force producing equilibrium, which Archimedes had possessed, and which was soon to reappear in Stevinus.

The properties of the lever had always continued known to mathematicians, although, in the dark period, the superiority of the proof given by Archimedes had not been recognised. We are not to be surprised, if reasonings like those of Jordanus were applied to demonstrate the theories of the lever with apparent success. Writers on mechanics were, as we have seen, so vacillating in their mode of dealing with words and propositions, that they would be made to prove anything which was already known to be true.

We proceed to speak of the beginning of the real progress of mechanics in modern times.

Sect. 2.—Revival of the Scientific Idea of Pressure.—Stevinus.—Equilibrium of Oblique Forces.

THE doctrine of the centre of gravity was the part of the speculations of Archimedes which was most diligently prosecuted after his time. Pappus and others, among the ancients, had solved some new problems on this subject, and Commandinus, in 1565, published *De Centro Gravitatis Solidorum*. Such treatises contained, for the most part, only mathematical consequences of the doctrines of Archimedes; but the mathematicians also retained a steady conviction of the mechanical property of the centre of gravity, namely, that all the weight of the body might be collected there, without any change in the mechanical results; a conviction which is closely connected with our fundamental concep-

tions of mechanical action. Such a principle, also, will enable us to determine the result of many simple mechanical arrangements; for instance, if a mathematician of those days had been asked whether a solid ball could be made of such a form, that, when placed on a horizontal plane, it should go on rolling forwards without limit, merely by the effect of its own weight, he would probably have answered, that it could not; for that the centre of gravity of the ball would seek the lowest position it could find, and that, when it had found this, the ball could have no tendency to roll any further. And, in making this assertion, the supposed reasoner would not be anticipating any wider proofs of the impossibility of a perpetual motion, drawn from principles subsequently discovered, but would be referring the question to certain fundamental convictions, which, whether put into axioms or not, inevitably accompany our mechanical conceptions.

In the same way, if Stevinus of Bruges, in 1586, when he published his *Beghinselen der Waaghconst* (Principles of Equilibrium), had been asked why a loop of chain, hung over a triangular beam, could not, as he asserted it could not, go on moving round and round perpetually, by the action of its own weight, he would probably have answered, that the weight of the chain, if it produced motion at all, must have a tendency to bring it into some certain position; and that when the chain had reached this position, it would have no tendency to go any further; and thus he would have reduced the impos-

sibility of such a perpetual motion, to the conception of gravity as a force tending to produce equilibrium, a principle perfectly sound and correct.

Upon this principle thus applied, Stevinus did establish the fundamental property of the inclined plane. He supposed a loop of string, loaded with fourteen equal balls at equal distances, to hang over a triangular support which had a horizontal base, and whose sides, being unequal in the proportion of two to one, supported four and two balls respectively. He showed that this loop must hang at rest, because any motion would only bring it into the same condition in which it was at first; and that the festoon of eight balls which hung down below the triangle might be removed without disturbing the equilibrium; so that four balls on the longer plane would balance two balls on the shorter planes, or the weights would be as the lengths of the planes intercepted by the horizontal line.

Stevinus showed his firm possession of the truth contained in this principle, by deducing from it the properties of forces acting in oblique directions under all kinds of conditions; in short, he showed his entire ability to found upon it a complete doctrine of equilibrium; and upon his foundations, and without any additional support, the mathematical doctrines of Statics might have been carried to the highest pitch of perfection they have yet reached. The formation of the science was finished; the mathematical developement and exposition of it were alone open to extension and change.

The contemporaneous progress of the other branch of mechanics, the Doctrine of Motion, interfered with this independent advance of Statics; and to that we must now turn. We may observe, however, that true propositions respecting the composition of forces appear to have rapidly diffused themselves. The *Tractatus de Motu* of Michael Varro of Geneva, already noticed, printed in 1584, had asserted, that the forces which balance each other, acting on the sides of a right-angled-triangular wedge, are in the proportion of the sides of the triangle; and although this assertion does not appear to have been derived from a distinct idea of pressure, the author had hence rightly deduced the properties of the wedge and the screw. And shortly after this time, Galileo also established the same results on different principles. In his Treatise *Delle Scienze Mechaniche* (1592), he refers the inclined plane to the lever, in a sound and nearly satisfactory manner; imagining a lever so placed, that the motion of a body at the extremity of one of its arms should be in the same direction as it is upon the plane. A slight modification makes this an unexceptionable proof.

Sect. 3.—*Prelude to the Science of Dynamics.—Attempts at the First Law of Motion.*

We have already seen, that Aristotle divided motions into natural and violent. Cardan endeavoured to improve this division by making three classes;

voluntary motion, which is circular and uniform, and which is intended to include the celestial motions; *natural* motion, which is stronger towards the end, as the motion of a falling body; and is in a straight line, because it is motion to an end, and nature seeks her ends by the shortest road: and thirdly, *violent* motion, including in this term all kinds different from the former two. Cardan was aware that such violent motion might be produced by a very small force; thus he asserts, that a spherical body resting on a horizontal plane may be put in motion by any force which is sufficient to cleave the air; for which, however, he erroneously assigns as a reason, the smallness of the point of contact[2]. But the most common mistake of this period was, that of supposing that as force is requisite to move a body, so a perpetual supply of force is requisite to keep it in motion. The whole of what Kepler called his "physical" reasoning, depended upon this assumption. He endeavoured to discover the forces by which the motions of the planets about the sun might be produced; but, in all cases, he considered the velocity of the planet as produced by, and exhibiting the effect of, a force which acted in the direction of the motion. Kepler's essays, which are in this respect so feeble and unmeaning, have sometimes been con-

[2] In speaking of the force which would draw a body up an inclined plane he observes, that "per communem animi sententiam," when the plane becomes horizontal, the requisite force is nothing.

sidered as disclosing some distant anticipation of Newton's discovery of the existence and law of central forces. There is however, in reality, no other connexion between these speculations than that which arises from the use of the term *force* by the two writers in two utterly different meanings. Kepler's forces were certain imaginary qualities which appeared in the actual motion which the bodies had; Newton's forces were causes which appeared by the change of motion: Kepler's forces urged the bodies forwards; Newton's deflected the bodies from such a progress. If Kepler's forces were destroyed, the body would instantly stop; if Newton's were annihilated, the body would go on uniformly in a straight line. Kepler compares the action of his forces to the way in which a body might be driven round, by being placed among the sails of a windmill; Newton's forces would be represented by a rope pulling the body to the centre. Newton's force is merely mutual attraction; Kepler's is something quite different from this; for though he perpetually illustrates his views by the example of a magnet, he warns us that the sun differs from the magnet in this respect, that its force is not attractive but directive[3]. Kepler's essays may with considerable reason be asserted to be an anticipation of the vortices of Descartes; but they can with no propriety whatever be said to anticipate Newton's dynamical theory.

[3] Epitome Astron. Copern. p. 170.

The confusion of thought which prevented mathematicians from seeing the difference between producing and preserving motion, was, indeed, fatal to all attempts at progress on this subject. We have already noticed the perplexity in which Aristotle involved himself, by his endeavours to find a reason for the continued motion of a stone after the moving power had ceased to act; and that he had ascribed it to the effect of the air or other medium in which the stone moves. Tartalea, whose *Nova Scienza* is dated 1551, though a good pure mathematician, is still quite in the dark on mechanical matters. One of his propositions, in the work just mentioned, is (B. i. Prop. 3), "The more a heavy body recedes from the beginning, or approaches the end of violent motion, the slower and more inertly it goes;" which he applies to the horizontal motion of projectiles. In like manner most other writers about this period conceived that a cannon-ball goes forwards till it loses all its projectile motion, and then falls downwards. Benedetti, who has already been mentioned, must be considered as one of the first enlightened opponents of this and other Aristotelian errors or puzzles. In his *Speculationum Liber*, (Venice, 1585,) he opposes Aristotle's mechanical opinions, with great expressions of respect, but in a very sweeping manner. His chapter xxiv. is headed, "Whether this eminent man was right in his opinion concerning violent and natural motion." And after stating the Aristotelian opinion just mentioned, that the body is

impelled by the air, he says that the air must impede rather than impel the body, and that[4] "the motion of the body, separately from the mover, arises by a certain natural impression from the impetuosity (ex impetuositate) received from the mover." He adds, that in natural motions this impetuosity continually increases, by the continued action of the cause,—namely, the propension of going to the place assigned it by nature; and that thus the velocity increases as the body moves from the beginning of its path. This statement shows a clearness of conception with regard to the cause of accelerated motion, which Galileo himself was long in acquiring.

Though Benedetti was thus on the way to the first law of motion, that all motion is uniform and rectilinear except so far as it is affected by extraneous forces, this law was not likely to be either generally conceived or satisfactorily proved, till the other laws of motion, by which the action of forces is regulated, had come into view. Hence, though a partial apprehension of this principle had preceded the discovery of the laws of motion, we must place the establishment of it in the period when those laws were detected and established, the period of Galileo and his followers.

[4] p. 184.

CHAPTER II.

INDUCTIVE EPOCH OF GALILEO.—DISCOVERY OF THE LAWS OF MOTION IN SIMPLE CASES.

Sect. 1.—*Establishment of the First Law of Motion.*

AFTER mathematicians had begun to doubt or reject the authority of Aristotle, they were still some time in coming to the conclusion, that the distinction of natural and violent motions was altogether untenable;—that the velocity of a body in motion increased or diminished in consequence of the action of causes, not of any property of the motion itself;—and that the apparently universal fact, of bodies going slower and slower, as if by their own disposition, till they finally stopped, from which motions had been called violent, arose from the action of external obstacles not immediately obvious, as the friction and the resistance of the air, when a ball runs on the ground, and the action of gravity, when it is thrown upwards. But the truth to which they were at last led, was, that such causes would account for *all* the diminution of velocity which bodies experience when apparently left to themselves; and that without such causes, the motion of all bodies would go on for ever, in a straight line and with a uniform velocity.

Who first announced this law in a general form it may be difficult to point out; its exact or approximate truth was necessarily taken for granted in all complete investigations on the subject of the laws of motion of falling bodies, and of bodies projected so as to describe curves. In Galileo's first attempt to solve the problem of falling bodies, he did not carry his analysis back to the notion of force, and therefore this law does not appear. In 1604 he had an erroneous opinion on this subject; and we do not know when he was led to the true doctrine which he published in his *Discorso*, in 1638. In his third Dialogue he gives the instance of water in a vessel, for the purpose of showing that circular motion has a tendency to continue. And in his first Dialogue on the Copernican System[1] (published in 1630), he asserts circular motion alone to be naturally uniform, and retains the distinction between natural and violent motion. In the Dialogues on Mechanics, however, published in 1638, but written apparently at an earlier period, in treating of projectiles[2], he asserts the true law. "Mobile super planum horizontale projectum mente concipio omni secluso impedimento, jam constat ex his quæ fusius alibi dicta sunt, illius motum equabilem et perpetuum super ipso plano futurum esse, si planum in infinitum extendatur." "Conceive a moveable body upon a horizontal plane, and suppose all obstacles to motion

[1] Dial. i. p. 40. [2] p. 141.

to be removed; it is then manifest, from what has been said more at large in another place, that the body's motion will be uniform and perpetual upon the plane, if the plane be indefinitely extended." His pupil, Borelli, in 1667 (in the treatise *De Vi Percussionis*), states the proposition generally, that "velocity is, by its nature, uniform and perpetual;" and this opinion appears to have been, at that time, generally diffused, as we find evidence in Wallis and others. It is commonly said that Descartes was the first to state this generally. His *Principia* were published in 1644; but his proofs of this first law of motion are rather of a theological than of a mechanical kind. His reason for this law is[s], "the immutability and simplicity of the operation by which God preserves motion in matter. For he only preserves it precisely as it is in that moment in which he preserves it, taking no account of that which may have been previously." Reasoning of this abstract and à priori kind, though it may be urged in favour of true opinions after they have been inductively established, is almost equally capable of being called in on the side of error, as we have seen in the case of Aristotle's philosophy. We ought not, however, to forget that the reference to these abstract and à priori principles is an indication of the absolute universality and necessity which we look for in complete sciences, and a result of those faculties by

[s] p. 34.

which such science is rendered possible, and suitable to man's intellectual nature.

The induction by which the first law of motion is established, consists, as induction consists in all cases, in conceiving clearly the law, and in perceiving the subordination of facts to it. But the law speaks of bodies not acted upon by any external force, a case which never occurs in fact; and the difficulty of the step consisted in bringing all the common cases in which motion is gradually extinguished, under the notion of the action of a retarding force. In order to do this, Hooke and others showed that, by diminishing the obvious resistances, the retardation also became less; and men were gradually led to a distinct appreciation of the resistance, friction, &c., which, in all terrestrial motions, prevent the law from being evident; and thus they at last established by experiment a law which cannot be experimentally exemplified. The natural uniformity of motion was proved by examining all kinds of cases in which it was not uniform. Men culled the abstract rule out of the concrete experiment; although the rule was, in every case, mixed with other rules, and each rule could be collected from the experiment only by supposing the others known. The perfect simplicity which we necessarily seek for in a law of nature, enables us to disentangle the complexity which this combination appears at first sight to occasion.

The first law of motion asserts that the motion of

a body, when left to itself, will not only be uniform, but rectilinear also. This latter part of the law is indeed obvious of itself, as soon as we conceive a body detached from all special reference to external points and objects. Yet, as we have seen, Galileo asserted that the naturally uniform motion of bodies was that which takes place in a circle. Benedetti, however, in 1585, had entertained sound notions on this subject. In commenting on Aristotle's question, why we obtain an advantage in throwing by using a sling, he says[4], that the body, when whirled round, tends to go on in a straight line. In Galileo's second Dialogue, he makes one of his interlocutors (Simplicio), when appealed to on this subject, after thinking intently for a little while, give the same opinion; and the principle is, from this time, taken for granted by the authors who treat of the motion of projectiles. Descartes, as might be supposed, gives the same reason for this as for the other part of the law, namely, the immutability of the Deity.

Sect. 2.—Formation and Application of the Notion of Accelerating Force.—Laws of Falling Bodies.

WE have seen how rude and vague were the attempts of Aristotle and his followers to obtain a philosophy of bodies falling downwards or thrown in any direction. If the first law of motion had been clearly known, it would then, perhaps, have been seen that

[4] "Corpus vellet rectâ iter peragere." Speculationum Liber, p. 160.

the way to understand and analyse the motion of any body, is to consider the causes of change of motion which at each instant operate upon it; and thus men would have been led to the notion of accelerating forces, that is, forces which act upon bodies already in motion, and accelerate, retard, or deflect their motions. It was, however, only after many attempts that they reached this point. They began by considering the whole motion with reference to certain ill-defined abstract notions, instead of considering, with a clear apprehension of the conditions of causation, the successive parts of which the motion consists. Thus, they spoke of the tendency of bodies to the centre, or to their own place, of projecting force, of impetus, of retraction, with little or no profit to knowledge. The indistinctness of their notions may, perhaps, be judged of from their speculations concerning projectiles. Santbach[5], in 1561, imagined that a body thrown with great velocity, as, for instance, a ball from a cannon, went in a straight line till all its velocity was exhausted, and then fell directly downwards. He has written a treatise on gunnery, founded on this absurd assumption. To this succeeded another doctrine, which, though not much more philosophical than the former, agreed much better with the phenomena. Nicolo Tartalea (*Nuova Scienza*, Venice,

[5] Problematum Astronomicorum et Geometricorum Sectiones, vii. &c. &c. Auctore Daniele Santbach, Noviomago. Basileæ, 1561.

1537; *Quesiti et Inventioni Diversi*, 1554) and Gualtier Rivius (*Architectura*, &c., Basil, 1582) represented the path of a cannon-ball as consisting, first of a straight line in the direction of the original projection, then of an arc of a circle in which it went on till its motion became vertical downwards, and then of a vertical line in which it continued to fall. The latter of these writers, however, was aware that the path must, from the first, be a curve; and treated it as a straight line, only because the curvature is very slight. Even Santbach's figure represents the path of the ball as partially descending before its final fall, but then it descends by *steps*, not in a curve. Santbach, therefore, did not conceive the *composition* of the effect of gravity with the existing motion, but supposed them to act alternately; Rivius, however, understood this composition, and saw that gravity must act as a deflecting force at every point of the path. Galileo, in his second Dialogue[s], makes Simplicius come to the same conclusion. "Since," he says, "there is nothing to support the body, when it quits that which projects it, it cannot be but that its proper gravity must operate," and it must immediately begin to decline downwards.

The force of gravity which thus produces deflection and curvature in the path of a body thrown obliquely, constantly increases the velocity of a body when it falls vertically downwards. The universality

[s] p. 147.

of this increase was obvious, both from reasoning and in fact; the law of it could only be discovered by closer consideration; and the full analysis of the problem required a distinct measure of the quantity of accelerating force. Galileo, who first solved this problem, began by viewing it as a question of fact, but conjectured the solution by taking for granted that the rule must be the simplest possible. "Bodies," he says[7], "will fall in the most simple way, because natural motions are always the most simple. When a stone falls, if we consider the matter attentively, we shall find that there is no addition, no increase, of the velocity more simple than that which is always added in the same manner," that is, when equal additions take place in equal times; "which we shall easily understand if we attend to the close connexion of motion and time." From this law, thus assumed, he deduced that the spaces described from the beginning of the motion must be as the squares of the times; and, again, assuming that the laws of descent for balls rolling down inclined planes, must be the same as for bodies falling freely, he verified this conclusion by experiment.

It will, perhaps, occur to the reader that this argument, from the simplicity of the assumed law, is somewhat insecure. It is not always easy for us to discern what that greatest simplicity is, which nature

[7] Dial. Sc. iv. p. 91.

adopts in her laws. Accordingly, Galileo was led wrong by this way of viewing the subject before he was led right. He at first supposed, that the velocity which the body had acquired at any point must be proportional to the *space* described from the point where the motion began. This false law is as simple in its enunciation as the true law, that the velocity is proportional to the *time:* it had been asserted as the true law by M. Varro (*De Motu Tractatus*, Genevæ, 1584), and by Baliani, a gentleman of Genoa, who published it in 1638. It was, however, soon rejected by Galileo, though it was afterwards taken up and defended by Casræus, one of Galileo's opponents. It so happens, indeed, that the false law is not only at variance with fact, but with itself; it involves a mathematical self-contradiction. This circumstance, however, was accidental: it would be easy to state laws of the increase of velocity which should be simple, and yet false in fact, though quite possible in their own nature.

The law of velocity was hitherto, as we have seen, treated as a law of phenomena, without reference to the causes of the law. "The cause of the acceleration of the motions of falling bodies is not," Galileo observes, "a necessary part of the investigation. Opinions are different. Some refer it to the approach to the centre; others say that there is a certain extension of the centrical medium, which, closing behind the body, pushes it forwards. For the present, it is enough for us to demonstrate certain pro-

perties of accelerated motion, the acceleration being according to the very simple law that the velocity is proportional to the time. And if we find that the properties of such motion are verified by the motions of bodies descending freely, we may suppose that the assumption agrees with the laws of bodies falling freely by the action of gravity[*]."

It was, however, an easy step to conceive this acceleration as caused by the continual action of gravity. This account had already been given by Benedetti, as we have seen. When it was once adopted, gravity was considered as a *constant* or *uniform* force; on this point, indeed, the adherents of the law of Galileo and of that of Casræus were agreed; but the question was, what is a uniform force? The answer which Galileo was obliged to give was obviously this;—*that* is a uniform force which generates equal velocities in equal successive times; and this principle leads at once to the doctrine, that forces are to be compared by comparing the velocities generated by them in equal times.

Though, however, this was a consequence of the rule by which gravity is represented as a uniform force, the subject presents some difficulty at first sight. It is not immediately obvious that we may thus measure forces by the velocity *added* in a given time, without taking into account the velocity they have already. If we communicate velocity to a body

[*] Gal. iii. 91, 92.

by the hand or by a spring, the effect we produce in a second of time is lessened, when the body has already a velocity which withdraws it from the pressure of the agent. But it appears that this is not so in the case of gravity; the velocity added in one second is the same, whatever downward motion the body already possesses. A body falling from rest acquires a velocity, in one second, of thirty-two feet; and if a cannon-ball were shot downwards with a velocity of 1000 feet a second, it would equally, at the end of one second, have received an accession of thirty-two feet to its velocity.

This conception of gravity as a uniform force,—as constantly and equally *increasing* the velocity of a descending body,—will become clear by a little attention; but it undoubtedly presents difficulty at first. Accordingly we find that Descartes did not accept it. "It is certain," he says, "that a stone is not equally disposed to receive a new motion or increase of velocity when it is already moving very quickly, and when it is moving slowly."

Descartes showed, by other expressions, that he had not caught hold of the true notion of accelerating force. Thus, he says, in a letter to Mersenne, "I am astonished at what you tell me, of having found, by experiment, that bodies thrown up in the air take neither more nor less time to rise than to fall again; and you will excuse me if I say that I look upon the experiment as a very difficult one to make accurately." Yet it is clear from the

notion of a constant force that (omitting the resistance of the air,) this equality must take place, for the force which will gradually destroy the whole velocity in a certain time in ascending, will, in the same time, generate again the same velocity by the same gradations inverted; and therefore the same space will be passed over in the same time in the descent and in the ascent.

Another difficulty arose from a necessary consequence of the laws of falling bodies thus established, namely, that in acquiring its motion; a body passes through every intermediate degree of velocity, from the smallest conceivable, up to that which it at last acquires. When a body falls from rest, it begins to fall with *no* velocity; the velocity increases with the time; and in one thousandth part of a second, the body has only acquired one thousandth part of the velocity which it has at the end of one second.

This is certain, and manifest on consideration; yet there was at first much difficulty raised on the subject of this assertion; and disputes took place concerning the velocity with which a body *begins* to fall. On this subject also Descartes did not form clear notions. He writes to a correspondent, "I have been revising my notes on Galileo, in which I have not said expressly that falling bodies do not pass through every degree of slowness, but I said that this cannot be known without knowing what weight is, which comes to the same thing; as to your example, I grant that it proves that every degree of velocity

is infinitely divisible, but not that a falling body actually passes through all these divisions."

The principles of the motion of falling bodies being thus established by Galileo, the deduction of the principal mathematical consequences was, as is usual, effected with great rapidity, and is to be found in his works and in those of his scholars and successors. The motion of bodies falling freely was, however, in such treatises, generally combined with the motion of bodies falling upon inclined planes; a part of the theory of which we have still to speak.

The notion of accelerating force and its operation, once formed, was naturally applied in other cases than that of bodies falling freely. The different velocities with which heavy and light bodies fall were explained by the different resistance of the air, which diminishes the accelerating force[9]; and it was boldly asserted, that in a vacuum a lock of wool and a piece of lead would fall equally quick. It was also maintained[10] that any falling body, however large and heavy, would always have its velocity in some degree diminished by the air in which it falls, and would at last be reduced to a state of uniform motion, as soon as the resistance upwards became equal to the accelerating force downwards. Though the law of progress of a body to this limiting velocity was not made out till the "Principia" of Newton appeared, the views on which Galileo made this

[9] Galileo, iii. 43. [10] iii. 54.

assertion are perfectly sound, and show that he had clearly conceived the nature and operation of accelerating and retarding force.

When uniform accelerating forces had once been mastered, there remained only mathematical difficulties in the treatment of variable forces. A variable force was measured by the *limit* of the increment of the velocity, compared with that of the time; just as a variable velocity was measured by the limit of the increment of the space compared with that of the time.

With this introduction of the notion of limits, we are, of course, led to the higher geometry, either in its geometrical or its analytical form. The general laws of bodies falling by the action of any variable forces, were given by Newton in the seventh Section of the "Principia." The subject is there, according to Newton's preference of geometrical methods, treated by means of the quadrature of curves, the doctrine of limits being exhibited in a peculiar manner in the first Section of the work, in order to prepare the way for such applications of it. Leibnitz, the Bernouillis, Euler, and since their time, many other mathematicians, have treated such questions by means of the analytical method of limits, the Differential Calculus. The rectilinear motion of bodies acted upon by variable forces is, of course, a simpler problem than their curvilinear motion, to which we have now to proceed. But it may be remarked that Newton, having established the laws of curvilinear

motion independently, has, in a great part of his seventh Section, deduced the simpler case of the rectilinear motion from the more complex problem, by reasonings of great ingenuity and beauty.

Sect. 3.—Establishment of the Second Law of Motion. Curvilinear Motions.

A SLIGHT degree of distinctness in men's mechanical notions enabled them to perceive, as we have already explained, that a body which traces a curved line must be urged by some force, by which it is constantly made to deviate from the rectilinear path, which it would pursue if acted upon by no force. Thus, when a body is made to describe a circle, as when a stone is whirled round in a sling, we find that the string does exert such a force on the stone, for it is stretched by the effort, and if it be too slender, it may thus be broken. This *centrifugal force* of bodies moving in circles was noticed even by the ancients. This effect of force to produce curvilinear motion also appears in the paths described by projectiles. We have already seen that though Tartalea did not perceive this correctly, Rivius, about the same time, did.

To see that the transverse force would produce a curve was one step; to determine what the curve is was another step, which involved the discovery of the second law of motion. This step was made by Galileo. In his Dialogues on Motion, he asserts

that a body projected horizontally will retain a uniform motion in the horizontal direction, and will have, compounded with this, a uniformly accelerated motion downwards; that is, the motion of a body falling vertically from rest, and will thus describe the curve called a parabola.

The second law of motion consists of this assertion in a general form;—that in all cases the motion which the force would produce is compounded with the motion which the body previously has. This was not obvious; for Cardan had maintained[11], that "if a body is moved by two motions at once, it will come to the place resulting from their composition slower than by either of them." The proof of the truth of the law to Galileo's mind was, so far as we collect from the dialogue itself, the simplicity of the supposition, and his clear perception of the causes which, in some cases, produced an obvious deviation in practice from this theoretical result. For it may be observed, that the curvilinear paths ascribed to military projectiles by Rivius and Tartalea, and by other writers who followed them, as Digges and Norton in our own country, though utterly different from the theoretical form, the parabola, do, in fact, approach nearer the true paths of a cannon or musket-ball than a parabola would do: and this approximation more especially exists in that which at first sight appears most absurd in the old theory; namely,

[11] Op. vol. iv. p. 490.

the assertion that the ball, which ascends in a sloping direction, finally descends vertically. In consequence of the resistance of the air, this is really the path of a projectile; and when the velocity is very great, as in military projectiles, the deviation from the parabolic form is very manifest. This cause of discrepancy between the theory, which does not take resistance into the account, and the fact, Galileo perceived; and accordingly he says[12], that the velocities of the projectiles, in such cases, may be considered as excessive and supernatural. With the due allowance to such causes, he maintained that his theory was verified, and might be applied in practice. This application no doubt had a share in confirming Galileo's views. We must not forget, however, that the establishment of this second law of motion was the result of the theoretical and experimental discussions concerning the motion of the earth: its fortunes were involved in those of the Copernican system; and it shared the triumph of that doctrine; which was already decisive, indeed, in the time of Galileo, but not complete till the time of Newton.

[12] Op. iii. 147.

Sect. 4.—*Generalisation of the Laws of Equilibrium.*—*Principle of Virtual Velocities.*

It was known, even as early as Aristotle, that the two weights which balance each other on the lever, if they move at all, move with velocities which are in the inverse proportion of the weights. The peculiar resources of the Greek language, which could state this relation of inverse proportionality in a single word (*ἀντιπέπονθεν*), fixed it in men's minds, and prompted them to generalise from this property. Such attempts were at first made with indistinct ideas, and on conjecture only; and had, therefore, no scientific value. This is the judgment which we must pass on the book of Jordanus Nemorarius, which we have already mentioned. Its reasonings are professedly on Aristotelian principles, and exhibit the common Aristotelian absence of all distinct mechanical ideas. But in Varro, whose *Tractatus de Motu* appeared in 1584, we find the principle, in a general form, not satisfactorily proved indeed, but much more distinctly conceived. This is his first theorem: "Duarum virium connexarum quarum (si moveantur) motus erunt ipsis *ἀντιπεπονθῶς* proportionales, neutra alteram movebit, sed equilibrium facient." The proof offered of this is, that the resistance to a force is as the motion produced; and, as we have seen, the theorem is rightly applied in the example of the wedge. From this time it appears to have been usual to prove the properties of machines by means

of this principle. This is done, for instance, in *Les Raisons des Forces Mouvantes*, the production of Solomon de Caus, engineer to the Elector Palatine, published at Antwerp in 1616; in which the effect of toothed-wheels and of the screw is determined in this manner, but the inclined plane is not treated of. The same is the case in Bishop Wilkins's *Mathematical Magic*, in 1648.

When the true doctrine of the inclined plane had been established, the laws of equilibrium for all the simple machines or mechanical powers, as they had usually been enumerated in books on mechanics, were brought into view; for it was easy to see that the *wedge* and the *screw* involved the same principle as the *inclined plane*, and the *pulley* could obviously be reduced to the *lever*. It was, also, not difficult for a person with clear mechanical ideas, to perceive how any other combination of bodies, on which pressure and traction are exerted, may be reduced to these simple machines, so as to disclose the relation of the forces. Hence by the discovery of Stevinus, all problems of equilibrium were essentially solved.

The conjectural generalisation of the property of the lever, which we have just mentioned, enabled mathematicians to express the solution of all these problems, by means of one proposition, This was done by saying, that in raising a weight by any machine, we *lose* in time what we *gain* in force; the weight raised moves as much *slower* than the power, as it is *larger* than the power. This was explained with great

clearness by Galileo, in the preface to his Treatise on Mechanical Science, published in 1592.

The motions, however, which we here suppose the parts of the machine to have, are not motions which the forces produce; for at present we are dealing with the case in which the forces balance each other, and therefore produce no motion. But we ascribe to the weights and powers hypothetical motions, arising from some other cause; and then, by the construction of the machine, the velocities of the weights and powers must have certain definite ratios. These velocities, being thus hypothetically supposed and not actually produced, are called *virtual* velocities. And the general law of equilibrium is, that in any machine, the weights which balance each other, are reciprocally to each other as their virtual velocities. This is called the *principle of virtual velocities.*

This principle (which was afterwards still further generalised) is, by some of the admirers of Galileo, dwelt upon as one of his great services to mechanics. But if we examine it more nearly, we shall see that it has not much importance in our history. It is a generalisation, but a generalisation established rather by enumeration of cases, than by any induction proceeding upon one distinct idea, like those generalisations of facts by which laws are primarily established. It rather serves verbally to conjoin laws previously known, than to exhibit a connexion in them: it is rather a help for the memory than a proof for the reason.

The principle of virtual velocities is so far from implying any clear possession of mechanical ideas, that any one who knows the property of the lever, whether he is capable of seeing the reason for it or not, can see that the greater weight moves slower in the exact proportion of its greater magnitude. Accordingly, Aristotle, whose entire want of sound mechanical views we have shown, has yet noticed this truth. When Galileo treats of it, instead of offering any reasons which could independently establish this principle, he gives a number of analogies and illustrations, many of them very loose. Thus the raising a great weight by a small force, is illustrated by supposing the weight broken into many small parts, and these raised one by one. By other persons, the analogy, already intimated, of gain and loss is referred to. Such images may please the fancy, but they cannot be accepted as mechanical reasons.

Since Galileo neither first enunciated this rule, nor ever proved it as an independent principle of mechanics, we cannot consider it as one of his mechanical achievements. Still less can we compare it with Stevinus's proof of the inclined plane; which, as we have seen, was rigorously inferred from the sound axiom, that a body cannot put itself in motion. If we were to assent to the really self-evident axioms of Stevinus, only in virtue of the unproved verbal generalisation of Galileo, we should be in great danger of allowing ourselves to be referred successively from one truth to another, with-

out any reasonable hope of ever arriving at anything ultimate and fundamental.

But though this principle cannot be looked upon as a great discovery of Galileo, it is a highly useful rule; and the varied forms under which he and his successors urged it, tended much to dissipate the vague wonder with which the effects of machines had been looked upon; and thus to diffuse sounder and clearer notions on such subjects.

This principle of virtual velocities also affected the progress of mechanical science in another way; it suggested some of the analogies by the aid of which the third law of motion was made out; leading to the adoption of the notion of *momentum* as the arithmetical product of weight and velocity. Since on a machine on which a weight of two pounds at one part balances three pounds at another part, the former weight would move through three inches while the latter would move through two inches; we see (since three multiplied into two is equal to two multiplied into three,) that the product of the weight and the velocity is the same for the two balancing weights: and if we call this product *momentum*, the law of equilibrium is, that when two weights balance on a machine, the momentum of each would be the same, if they were put in motion.

The notion of momentum was here employed in connexion with virtual velocities: but it also came under consideration in treating of actual velocities, as we shall soon see.

Sect. 5.—*Attempts at the Third Law of Motion.—Notion of Momentum.*

In the questions we have hitherto had to consider respecting motion, no regard is had to the size of the body moved, but only to the velocity and direction of the motion. We must now trace the progress of knowledge respecting the mode in which the mass of the body influences the effect of force. This is a more difficult and complex branch of the subject; but it is one which requires to be noticed, as obviously as the former. Questions belonging to this department of mechanics, as well as to the others, occur in Aristotle's Mechanical Problems. "Why," says he, "is it, that neither very small nor very large bodies go far when we throw them; but, in order that this may happen, the thing thrown must have a certain proportion to the agent which throws it? Is it that what is thrown or pushed must react[13] against that which pushes it; and that a body so large as not to yield at all, or so small as to yield entirely, and not to react, produces no throw or push?" The same confusion of ideas prevailed after his time: and mechanical questions were in vain discussed by means of general and abstract terms, employed with no distinct and steady meaning; such as *impetus*, *power*, *momentum*, *virtue*, *energy*, and the like. From

[13] ἀντερείδειν.

some of these speculations we may judge how thorough the confusion in men's heads had become. Cardan perplexes himself with the difficulty, already mentioned, of the comparison of the forces of bodies at rest and in motion. If the force of a body depends on its velocity, as it appears to do, how is it that a body at rest has any force at all, and how can it resist the slightest effort, or exert any pressure? He flatters himself that he solves this question, by asserting that bodies at rest have an occult motion. "Corpus movetur occulto motu quiescendo."— Another puzzle, with which he appears to distress himself rather more wantonly, is this: "If one man can draw half of a certain weight, and another man also one half; when the two act together, these proportions should be compounded; so that they ought to be able to draw one half of one half, or one quarter only." The talent which ingenious men had for getting into such perplexities, was certainly at one time very great. Arriaga[14], who wrote in 1639, is troubled to discover how several flat weights, lying one upon another on a board, should produce a greater pressure together than the lowest one alone produces, since that alone touches the board. Among other solutions, he suggests that the board affects the upper weight, which it does not touch, by determining its *ubication*, or *whereness*.

Aristotle's doctrine, that a body ten times as heavy

[14] Rod. de Arriaga, *Cursus Philosophicus*, Paris, 1639.

as another, will fall ten times as fast, is another instance of the confusion of statical and dynamical forces: the force of the greater body, while at rest, is ten times as great as that of the other; but the force, as measured by the velocity produced, is equal in the two cases. The two bodies would fall downwards with the same rapidity, except so far as they are affected by accidental causes. The merit of proving this by experiment, and thus refuting the Aristotelian dogma, is usually ascribed to Galileo, who made his experiment from the famous leaning tower of Pisa, about 1590. But others about the same time had not overlooked so obvious a fact.—F. Piccolomini, in his *Liber Scientiæ de Natura*, published at Padua, in 1597, says, "On the subject of the motion of heavy and light bodies, Aristotle has put forth various opinions, which are contrary to sense and experience, and has delivered rules concerning the proportion of quickness and slowness, which are palpably false. For a stone twice as great does *not* move twice as fast." And Stevinus, in the Appendix to his Statics, describes his having made the experiment, and speaks with great correctness of the apparent deviations from the rule, arising from the resistance of the air. Indeed, the result followed by very obvious reasoning; for ten bricks, in contact with each other, would obviously fall in the same time as one; and these might be conceived to form a body ten times as large as one of them. Accordingly, Benedetti, in 1585, reasons in this manner

with regard to bodies of different size, though he retains Aristotle's error as to the different velocity of bodies of different density.

The next step in this subject is more clearly due to Galileo; he discovered the true proportion which the accelerating force of a body falling down an inclined plane bears to the accelerating force of the same body falling freely. This was at first a happy conjecture; it was then confirmed by experiments, and, finally, after some hesitation, it was referred to its true principle, the third law of motion, with proper elementary simplicity. The principle here spoken of is this; that for the same body, the dynamical effect of force is as the statical effect; that is, the velocity which any force generates in a given time when it puts the body in motion, is proportional to the pressure which the same force produces in a body at rest. The principle, so stated, appears very simple and obvious; yet this was not the form in which it suggested itself either to Galileo or to other persons who sought to prove it. Galileo, in his Dialogues on Motion, assumes, as his fundamental proposition on this subject, one much less evident than that we have quoted, but one in which that is involved. His postulate is[15], that when the same body falls down different planes of the same height, the velocities acquired are equal. He confirms and illustrates this by a very ingenious experiment on a

[15] Opere, iii. 96.

pendulum, showing that the weight swings to the same height whatever path it be compelled to follow. Torricelli, in his treatise published 1644, says that he had heard that Galileo had, toward the end of his life, proved his assumption, but that, not having seen the proof, he will give his own. In this he refers us to the right principle, but appears not distinctly to conceive the proof, since he estimates *momentum* indiscriminately by the statical pressure of a body, and by its velocity when in motion; as if these two quantities were self-evidently equal. Huyghens, in 1673, expresses himself dissatisfied with the proof by which Galileo's assumption was supported in the later editions of his works. His own proof rests on this principle; that if a body fall down one inclined plane, and proceed up another with the velocity thus acquired, it cannot, under any circumstances, ascend to a higher position than that from which it fell. This principle coincides very nearly with Galileo's experimental illustration. In truth, however, Galileo's principle, which Huyghens thus slights, may be looked upon as a satisfactory statement of the true law; namely, that, in the same body, the velocity produced is as the pressure which produces it. "We are agreed," he says[16], "that, in a moveable body, the *impetus, energy, momentum,* or *propension to motion,* is as great as is the *force* or *least resistance* which suffices to *support* it." The various terms here used,

[16] Galileo, iii. 104.

both for dynamical and statical force, show that Galileo's ideas were not confused by the ambiguity of any one term, as appears to have happened to some mathematicians. The principle thus announced, is, as we shall see, one of great extent and value; and we read with interest the circumstances of its discovery, which are thus narrated[17]. When Viviani was studying with Galileo, he expressed his dissatisfaction at the want of any clear reason for Galileo's postulate respecting the equality of velocities acquired down inclined planes of the same heights; the consequence of which was, that Galileo, as he lay, the same night, sleepless through indisposition, discovered the proof which he had long sought in vain, and introduced it in the subsequent editions. It is easy to see, by looking at the proof, that the discoverer had had to struggle, not for intermediate steps of reasoning between remote notions, as in a problem of geometry, but for a clear possession of ideas which were near each other, and which yet could not be brought into contact, because he had not yet a sufficiently firm grasp of them. Such terms as momentum and force had been sources of confusion from the time of Aristotle; and it required considerable steadiness of thought to compare the forces of bodies at rest and in motion under the obscurity and vacillation thus produced.

The term *momentum* had been introduced to

[17] Drinkwater, Life of Galileo, p. 59.

express the force of bodies in motion before it was known what that effect was. Galileo, in his *Discorso intorno alle Cose che stanno in su l'Acqua*, says, that "momentum is the force, efficacy, or virtue, with which the motion moves and the body moved resists, depending not upon weight only, but upon the velocity, inclination, and any other cause of such virtue." When he arrived at more precision in his views, he determined, as we have seen, that, in the same body, the momentum is *proportional* to the velocity; and, hence it was easily seen that in different bodies it was proportional to the velocity and mass jointly. The principle thus enunciated is capable of very extensive application, and, among other consequences, leads to a determination of the results of the mutual percussion of bodies. But though Galileo, like others of his predecessors and contemporaries, had speculated concerning the problem of percussion, he did not arrive at any satisfactory conclusion; and the problem remained for the mathematicians of the next generation to solve.

We may here notice Descartes and his Laws of Motion, the publication of which is sometimes spoken of as an important event in the history of mechanics. This is saying far too much. The *Principia* of Descartes did little for physical science. His assertion of the laws of motion, in their most general shape, was perhaps an improvement in form; but his third law is false in substance. Descartes claimed several

of the discoveries of Galileo and others of his contemporaries; but we cannot assent to such claims, when we find that, as we shall see, he did not understand, or would not apply, the laws of motion when he had them before him. If we were to compare Descartes with Galileo, we might say, that of the mechanical truths which were easily attainable in the beginning of the seventeenth century, Galileo took hold of as many, and Descartes of as few, as was well possible for a man of genius.

CHAPTER III.

SEQUEL TO THE EPOCH OF GALILEO.—PERIOD OF VERIFICATION AND DEDUCTION.

THE evidence on which Galileo rested the truth of the laws of motion which he asserted, was, as we have seen, the simplicity of the laws themselves, and the agreement of their consequences with facts; proper allowances being made for disturbing causes. His successors took up and continued the task of making repeated comparisons of the theory and practice, till no doubt remained of the exactness of the fundamental doctrines: they also employed themselves in simplifying, as much as possible, the mode of stating these doctrines, and in tracing their consequences in various problems by the aid of mathematical reasoning. These employments led to the publication of various Treatises on falling bodies, inclined planes, pendulums, projectiles, spouting fluids, which occupied a great part of the seventeenth century.

The authors of these treatises may be considered as the School of Galileo. Several of them were, indeed, his pupils or personal friends. Castelli was his disciple and astronomical assistant at Florence, and afterwards his correspondent. Torricelli was at first a pupil of Castelli, but became the inmate and

amanuensis of Galileo in 1641, and succeeded him in his situation at the court of Florence on his death, which took place a few months afterwards. Viviani formed one of his family during the three last years of his life, and surviving him and his contemporaries, (for Viviani lived even into the eighteenth century,) has a manifest pleasure and pride in calling himself the last of the disciples of Galileo. Gassendi, an eminent French mathematician and professor, visited him in 1628; and it shows us the extent of his reputation when we find Milton referring thus to his travels in Italy[1]: "There it was that I found and visited the famous Galileo, grown old, a prisoner in the Inquisition, for thinking in astronomy otherwise than the Franciscan and Dominican licensers thought."

Besides the above writers, we may mention, as persons who pursued and illustrated Galileo's doctrines, Borelli, who was professor at Florence and Pisa; Mersenne, the correspondent of Descartes, who was professor at Paris; Wallis, who was appointed Savilian professor at Oxford in 1649, his predecessor being ejected by the parliamentary commissioners. It is not necessary for us to trace the progress of purely mathematical inventions, which constitute a great part of their works; but a few circumstances may be mentioned.

The question of the proof of the second law of

[1] Speech for the Liberty of Unlicensed Printing.

motion was, from the first, identified with the controversy respecting the truth of the Copernican system; for this law supplied the true answer to the most formidable of the objections against the motion of the earth; namely, that bodies which were dropt from an elevated object would be left behind by the place from which they fell. This argument was reproduced in various forms by the opponents of the new doctrine; and the answers to the argument, though they belong to the history of astronomy, and form part of the sequel to the epoch of Copernicus, belong more peculiarly to the history of mechanics, and are events in the sequel to the discoveries of Galileo. So far, indeed, as the mechanical controversy was concerned, the advocates of the second law of motion appealed, very triumphantly, to experiment. Gassendi made many experiments on this subject publicly, of which an account is given in his *Epistolæ tres de Motu Impresso a Motore Translato*[s]. It appeared in these experiments that bodies let fall downwards, or cast upwards, forwards, or backwards, from a ship, or chariot, or man, whether at rest, or in any degree of motion, had always the same motion relatively to the *motor*. In the application of this principle to the system of the world, indeed, Gassendi and other philosophers of his time were greatly hampered; for the deference which religious scruples required, did not allow them to say that the earth

[s] Mont. ii. 199.

really moved, but only that the physical reasons against its motion were invalid. This restriction enabled Riccioli and other writers on the geocentric side to involve the subject in metaphysical difficulties; but the conviction of men was not permanently shaken by these, and the second law of motion was soon assumed as unquestioned.

The laws of the motion of falling bodies, as assigned by Galileo, were confirmed by the reasonings of Gassendi and Fermat, and the experiments of Riccioli and Grimaldi; and the effect of resistance was pointed out by Mersenne and Dechales. The parabolic motion of projectiles was more especially illustrated by experiments on the jet which spouts from an orifice in a vessel full of fluid. This mode of experimenting is well adapted to attract notice, since the curve described, which is transient and invisible in the case of a single projectile, becomes permanent and visible when we have a continuous stream. The doctrine of the motions of fluids has always been zealously cultivated by the Italians. Castelli's treatise, *Della Misura dell' Acque Corrente*, (1638,) is the first on this subject, and Montucla with justice calls him "the creator of a new branch of hydraulics[3];" although he mistakenly supposed the velocity of efflux to be as the depth of the orifice from the surface. Mersenne and Torricelli also pursued this subject, and after them many others.

[3] ii. 201.

Galileo's belief in the near approximation of the curve described by a cannon-ball or musket-ball to the theoretical parabola, was somewhat too obsequiously adopted by succeeding practical writers on artillery. They underrated, as he had done, the effect of the resistance of the air, which is in fact so great as entirely to change the form and properties of the curve. Notwithstanding this, the parabolic theory was employed, as in Anderson's *Art of Gunnery;* (1674;) and Blondel, in his *Art de jeter les Bombes*, (1683,) not only calculated tables on this supposition, but attempted to answer the objections which had been made respecting the form of the curve described. It was not till a later period, (1740,) when Robins made a series of careful and sagacious experiments on artillery, and when some of the most eminent mathematicians calculated the curve, taking into account the resistance, that the theory of projectiles could be said to be verified in fact.

The third law of motion was still in some confusion when Galileo died, as we have seen. The next great step made in the school of Galileo was the determination of the laws of the motions of bodies in their direct impact, so far as this impact affects the motion of translation. The difficulties of the problem of percussion arose, in part, from the heterogeneous nature of pressure (of a body at rest), and momentum (of a body in motion); and, in part, from mixing together the effects of percussion on

the parts of a body, as, for instance, cutting, bruising, and breaking, with its effect in moving the whole.

The former difficulty had been seen with some clearness by Galileo himself. In a posthumous addition to his mechanical dialogues, he says, "there are two kinds of resistance in a moveable body, one internal, as when we say it is more difficult to lift a weight of a thousand pounds than a weight of a hundred; another respecting space, as when we say that it requires more force to throw a stone one hundred paces than fifty[4]." Reasoning upon this difference, he comes to the conclusion that "the momentum of percussion is infinite, since there is no resistance, however great, which is not overcome by a force of percussion, however small[5]." He further explains this by observing that the resistance to percussion must occupy some portion of time, although this portion may be insensible. This correct mode of removing the apparent incongruity of continuous and instantaneous force, was a material step in the solution of the problem.

The laws of the mutual impact of bodies were erroneously given by Descartes in his *Principia;* and appear to have been first correctly stated by Wren, Wallis, and Huyghens, who about the same time (1669,) sent papers to the Royal Society of London on the subject. In these solutions, we perceive that men were gradually coming to apprehend

[4] Op. iii. 210. [5] iii. 211.

the third law of motion in its most general sense; namely, that the momentum (which is proportional to the mass of the body and its velocity jointly,) may be taken, the measure of the effect; so that this momentum is as much diminished in the striking body by the resistance it experiences, as it was increased in the body struck by the impact. This was sometimes expressed by saying that the quantity of motion remains unaltered, *quantity of motion* being used as synonymous with *momentum*. Newton expressed it by saying that "action and reaction are equal and opposite," which is still one of the most familiar modes of expressing the third law of motion.

In this mode of stating the law, we see an example of a propensity which has prevailed very generally among mathematicians; namely, a disposition to present the fundamental laws of rest and motion as if they were equally manifest, and, indeed, identical. The close analogy and connexion which exists between the principles of equilibrium and of motion, often led men to confound their evidence; and this confusion introduced an ambiguity in the use of words, as we have seen in the case of momentum, force, and others. The same may be said of *action* and *reaction*, which have both a statical and a dynamical signification. And by this means, the most general statements of the laws of motion are made to coincide with the most general statical propositions. For instance, Newton deduced from his

principles the conclusion, that by the mutual action of bodies, the motion of their centre of gravity cannot be affected. Marriotte, in his *Traité de la Percussion* (1684), had asserted this proposition for the case of direct impact. But by the reasoners of Newton's time, the dynamical proposition, that the motion of the centre of gravity is not altered by the *actual* free motion and impact of bodies, was associated with the statical proposition, that when bodies are in equilibrium, the centre of gravity cannot be made to ascend or descend by the *virtual* motions of the bodies. This latter is a proposition which was assumed as self-evident by Torricelli; but which may more philosophically be proved from elementary statical principles.

This disposition to identify the elementary laws of equilibrium and of motion, led men to think too slightingly of the ancient solid and sufficient foundation of statics, the doctrine of the lever. When the progress of thought had opened men's minds to a more general view of the subject, it was considered as a blemish in the science to found it on the properties of one particular machine. Descartes says in his Letters, that "it is ridiculous to prove the pulley by means of the lever." And Varignon was led by similar reflections to the project of his *Nouvelle Mécanique*, in which the whole of statics should be founded on the composition of forces. This project was published in 1687; but the work did not appear till after the death of the author. Though the

attempt to reduce the equilibrium of all machines to the composition of forces, is philosophical and meritorious, the attempt to reduce the composition of pressures to the composition of *motions*, with which Varignon's work is occupied, was a retrograde step in the subject, so far as the progress of distinct mechanical ideas was concerned.

Thus, at the period at which we have now arrived, the principles of elementary mechanics were generally known and accepted; and there was in the minds of mathematicians a prevalent tendency to reduce them to the most simple and comprehensive form of which they admitted. The execution of this simplification and extension, which we term the generalisation of the laws, is so important an event, that though it forms part of the natural sequel of Galileo, we shall treat of it in a separate chapter. But we must first bring up the history of the mechanics of fluids to the corresponding point.

CHAPTER IV.

Discovery of the Mechanical Principles of Fluids.

Sect. 1.—*Rediscovery of the Laws of Equilibrium of Fluids.*

We have already said, that the true laws of the equilibrium of fluids were discovered by Archimedes, and rediscovered by Galileo and Stevinus; the intermediate time having been occupied by a vagueness and confusion of thought on physical subjects, which made it impossible for men to retain such clear views as Archimedes had disclosed. Stevinus must be considered as the earliest of the authors of this rediscovery; for his work was published in Dutch about 1585; and in this, his views are perfectly distinct and correct. He restates the doctrines of Archimedes, and shows that, as a consequence of them, it follows that the pressure of a fluid on the bottom of a vessel may be much greater than the weight of the fluid itself: this he proves, by imagining some of the upper portions of the vessel to be filled with fixed solid bodies, which take the place of the fluid, and yet do not alter the pressure on the base. He also shows what will be the pressure on any

portion of a base in an oblique position; and hence, by certain mathematical artifices which make an approach to the infinitesimal calculus, he finds the whole pressure on the base in such cases. This mode of treating the subject would take in a large portion of our elementary hydrostatics as it now stands. Galileo saw the properties of fluids no less clearly, and explained them very distinctly, in 1612, in his Discourse on Floating Bodies. It had been maintained by the Aristotelians, that form was the cause of bodies floating; and collaterally, that ice was condensed water; apparently from a confusion of thought as to rigidity and density. Galileo asserted, on the contrary, that ice is *rarefied* water, as appears by its floating; and in support of this, he proved, by various experiments, that the floating of bodies does not depend on their form. The happy genius of Galileo is the more remarkable in this case, as the controversy was a good deal perplexed by the mixture of phenomena of another kind, due to what is usually called capillary or molecular attraction. Thus it is a fact, that a *ball* of ebony sinks in water, while a *flat slip* of the same material lies on the surface; and it required considerable sagacity to separate such cases from the general rule. Galileo's opinions were attacked by various writers, as Nozzolini, Vincenzio di Grazia, Ludovico delle Colombe; and defended by his pupil Castelli, who published a reply in 1615. These opinions were generally adopted and diffused; but somewhat later,

Pascal pursued the subject more systematically, and wrote his *Treatise of the Equilibrium of Fluids* in 1653; in which he shows that a fluid, enclosed in a vessel, necessarily presses equally in all directions, by imagining two *pistons*, or sliding plugs, applied at different parts, the surface of one being centuple that of the other: it is clear, as he observes, that the force of one man acting at the first piston, will balance the force of one hundred men acting at the other. "And thus," says he, "it appears that a vessel full of water is a new principle of mechanics, and a new machine which will multiply force to any degree we choose." He also referred the equilibrium of fluids to the "principle of virtual velocities," which regulates the equilibrium of other machines. This, indeed, Galileo had done before him. It followed from this doctrine, that the pressure which is exercised by the lower parts of a fluid arises from the weight of the upper parts.

In all this there was nothing which was not easily assented to: but the extension of these doctrines to the air required an additional effort of mechanical conception. The pressure of the air on all sides of us, and its weight above us, were two truths which had never yet been apprehended with any kind of clearness. Seneca, indeed[1], talks of the "gravity of the air," and of its power of diffusing itself when condensed, as the causes of wind: but we can hardly

[1] Quæst. Nat. v. 5.

consider such propriety of phraseology in him as more than a chance; for we see the value of his philosophy by what he immediately adds: "Do you think that we have forces by which we move ourselves, and that the air is left without any power of moving? when even water has a motion of its own, as we see in the growth of plants." We can hardly attach much value to such a recognition of the gravity and elasticity of the air.

Yet the effects of these causes were so numerous and obvious, that the Aristotelians had been obliged to invent a principle to account for them; namely, "nature's horror of a vacuum." To this principle were referred many familiar phenomena, as suction, breathing, the action of a pair of bellows, its drawing water if immersed in water, its refusing to open when the vent is stopped up. The action of a cupping instrument, in which the air is rarefied by fire; the fact that water is supported when a full inverted bottle is placed in a basin; or when a full tube, open below and closed above, is similarly placed; the running out of the water, in this instance, when the top is opened; the action of a siphon, a syringe, a pump; the adhesion of two polished plates, and other facts, were all explained by the *fuga vacui*. Indeed, we must contend that the principle was a very good one, inasmuch as it brought together all these facts, which are really of the same kind, and referred them to a common cause. But when urged as an ultimate principle, it was not only

unphilosophical, but *imperfect* and *wrong*. It was *unphilosophical*, because it introduced the notion of an emotion, horror, as an account of physical facts; it was *imperfect*, because it was at best only a law of phenomena, not pointing out any physical cause; and it was *wrong*, because it gave an unlimited extent to the effect. Accordingly, it led to mistakes. Thus Mersenne, in 1644, speaks of a siphon which shall go over a mountain, being ignorant then that the effect of such an instrument was limited to thirty-four feet. A few years later, however, he had detected this mistake; and in his third volume, published in 1647, he puts his siphon in his *emendanda*, and speaks correctly of the weight of air as supporting the mercury in the tube of Torricelli. It was, indeed, by the limit of this horror of a vacuum to the height of thirty-four feet, that the true principle was suggested. It was found that when attempts were made to raise water higher than this, nature tolerated a vacuum above the water which rose. In 1643, Torricelli tried to produce this vacuum at a smaller height, by using, instead of water, the heavier fluid, quicksilver; an attempt which shows that the true explanation, the balance of the weight of the water by another pressure, had already suggested itself. Indeed, this appears from other evidence. Galileo had already taught that the air has weight; and Baliani, writing to him in 1630, says[1], "If we were in a vacuum, the weight of the

[1] Drinkwater's Galileo, p. 90.

air above our heads would be felt." Descartes also appears to have some share in this discovery; for, in a letter of the date of 1631, he explains the suspension of mercury in a tube, closed at top, by the pressure of the column of air reaching to the clouds.

Still men's minds wanted confirmation in this view: they found such confirmation, when, in 1647, Pascal showed practically, that if we alter the length of the superincumbent column of air by going to a high place, we alter the weight which it will support. This celebrated experiment was made by Pascal himself on a church steeple in Paris, the column of mercury in the Torricellian tube being used to compare the weights of the air; but he wrote to his brother-in-law, who lived near the high mountain of Puy de Dome in Auvergne, to request him to make the experiment there, where the result would be more decisive. "You see," he says, "that if it happens that the height of the mercury at the top of the hill be less than at the bottom, (which I have many reasons to believe, though all those who have thought about it are of a different opinion,) it will follow that the weight and pressure of the air are the sole cause of this suspension, and not the horror of a vacuum: since it is very certain that there is more air to weigh on it at the bottom than at the top; while we cannot say that nature abhors a vacuum at the foot of a mountain more than on its summit."— M. Perrier, Pascal's correspondent, made the observation as he had desired, and found a difference of

three inches of mercury, "which," he says, "ravished us with admiration and astonishment."

When the least obvious case of the operation of the pressure and weight of fluids had thus been made out, there were no further difficulties in the progress of the theory. When mathematicians began to consider more general cases than those of the action of gravity, there arose differences in the way of stating the appropriate principles: but none of these differences imply any different conception of the fundamental nature of fluid equilibrium.

Sect. 2.—*Discovery of the Laws of Motion of Fluids.*

THE art of conducting water in pipes, and of directing its motion for various purposes, is very old. When treated systematically, it has been termed *Hydraulics:* but *Hydrodynamics* is the general name of the science of the laws of the motions of fluids, under those or other circumstances. The art is as old as the commencement of civilization: the science does not ascend higher than the time of Newton, though attempts on such subjects were made by Galileo and his scholars.

When a fluid spouts from an orifice in a vessel, Castelli saw that the velocity of efflux depends on the depth of the orifice below the surface: but he erroneously judged the velocity to be exactly proportional to the depth. Torricelli found that the fluid, under the inevitable causes of defect which occur in

the experiment, would spout nearly to the height of the surface: he therefore inferred, that the full velocity is that which a body would acquire in falling through the depth; and that it is consequently proportional to the square root of the depth.—This, however, he stated only as a result of experience, or law of phenomena, at the end of his treatise, *De Motu Naturaliter Accelerato*, printed in 1643.

Newton treated the subject theoretically in the Principia (1687); but we must allow, as Lagrange says, that this is the least satisfactory passage of that great work. Newton, having made his experiments in another manner than Torricelli, namely, by measuring the *quantity* of the efflux instead of its velocity, found a result inconsistent with that of Torricelli. The velocity inferred from the quantity discharged, was only that due to *half* the depth of the fluid.

In the first edition of the Principia[3], Newton gave a train of reasoning by which he theoretically demonstrated his own result, going upon the principle, that the momentum of the issuing fluid is equal to the momentum which the column vertically over the orifice would generate by its gravity. But Torricelli's experiments, which had given the velocity due to the whole depth, were confirmed on repetition: how was this discrepancy to be explained?

[3] B. ii. Prop. xxxvii.

Newton explained it by observing the contraction which the jet, or vein of water, undergoes, just after it leaves the orifice, and which he called the *vena contracta.* At the orifice, the velocity is that due to half the height; at the vena contracta it is that due to the whole height. The former velocity regulates the quantity of the discharge; the latter, the path of the jet.

This explanation was an important step in the subject: but it made Newton's original proof appear very defective, to say the least. In the second edition of the Principia (1714), Newton attacked the problem in a manner altogether different from his former one. He there assumes, that when a round vessel, containing fluid, has a hole in its bottom, the descending fluid may be conceived to be a conoidal mass, which has its base at the surface of the fluid, and its narrow end at the orifice. This portion of the fluid he calls the *cataract;* and supposes that while this part descends, the surrounding parts remain immovable, as if they were frozen; in this way he finds a result agreeing with Torricelli's experiments on the velocity of the efflux.

We must allow that the assumptions by which this result is obtained are somewhat arbitrary; and those which Newton introduces in attempting to connect the problem of issuing fluids with that of the resistance to a body moving in a fluid, are no less so. But even up to the present time, mathematicians have not been able to reduce problems

concerning the motions of fluids to mathematical principles and calculation, without introducing some steps of this arbitrary kind. And one of the uses of experiment on this subject is, to suggest those hypotheses which may enable us, in the manner most consonant with the true state of things, to reduce the motions of fluids to those general laws of mechanics, to which we know they must be subject.

Hence the science of the motion of fluids, unlike all the other primary departments of mechanics, is a subject on which we still need experiments, to point out the fundamental principles. Many such experiments have been made, with a view either to compare the results of deduction and observation, or, when this comparison failed, to obtain purely empirical rules. In this way the resistance of fluids, and the motion of water in pipes, canals, and rivers, has been treated. Italy has possessed, from early times, a large body of such writers. The earlier works have been collected in sixteen quarto volumes. Lecchi and Michelotti about 1765, Bidone more recently, have pursued these inquiries. Bossut, Buat, Hachette, in France, have laboured at the same task, as have Coulomb and Prony, Girard and Poncelet. Eytelwein's German treatise (Hydraulik), contains an account of what others and himself have done. Many of these trains of experiments, both in France and Italy, were made at the expense of governments, and on a very magnificent scale. In England less was done in this way during the last century, than in most other

countries. The Philosophical Transactions, for instance, scarcely contain a single paper on this subject founded on experimental investigations[4]. Dr. Thomas Young, who was at the head of his countrymen in so many branches of science, was one of the first to call back attention to this: and Mr. Rennie and others have recently made valuable experiments. In many of the questions now spoken of, the accordance which engineers are able to obtain, between their calculated and observed results, is very great: but these calculations are performed by means of empirical formulæ, which do not connect the facts with their causes, and still leave a wide space to be traversed, in order to complete the science.

In the mean time, all the other portions of mechanics were reduced to general laws, and analytical processes; and means were found of including Hydrodynamics, notwithstanding the difficulties which attend its special problems, in this common improvement of form; as we must now relate.

[4] Rennie, Report to Brit. Assoc.

CHAPTER V.

GENERALISATION OF THE PRINCIPLES OF MECHANICS.

Sect. 1.—*Generalisation of the Second Law of Motion. Central Forces.*

THE second law of motion being proved for constant forces which act in parallel lines, and the third law for the direct action of bodies, it still required great mathematical talent, and some inductive power, to see clearly the laws which govern the motion of any number of bodies, acted upon by each other, and by any forces, anyhow varying in magnitude and direction. This was the task of the generalisation of the laws of motion.

Galileo had convinced himself that the velocity of projection, and that which gravity alone would produce, are "both maintained, without being altered, perturbed, or impeded in their mixture." It is to be observed, however, that the truth of this result depends upon a particular circumstance, namely, that gravity, at all points, acts in lines, which, as to sense, are parallel. When we have to consider cases in which this is not true, as when the force tends to the centre of a circle, the law of composition cannot be

applied in the same way; and, in this case, mathematicians were met by some peculiar difficulties.

One of these difficulties arises from the apparent inconsistency of the statical and dynamical measures of force. When a body moves in a circle, the force which urges the body to the centre is only a *tendency* to motion; for the body does not, in fact, approach to the centre; and this mere tendency to motion is combined with an actual motion, which takes place in the circumference. We appear to have to compare two things which are heterogeneous. Descartes has noticed this difficulty, but without giving any satisfactory solution of it[1]. If we combine the actual motion to or from the centre with the transverse motion about the centre, we obtain a result which is false on mechanical principles. Galileo endeavoured in this way to find the curve described by a body which falls towards the earth's centre, and is, at the same time, carried round by the motion of the earth; and obtained an erroneous result. Kepler and Fermat attempted the same problem, and obtained solutions different from that of Galileo, but not more correct.

Even Newton, at an early period of his speculations, had an erroneous opinion respecting this curve, which he imagined to be a kind of spiral. Hooke animadverted upon this opinion when it was laid before the Royal Society of London in 1679,

[1] Princip. P. iii. 59.

and stated, more truly, that, supposing no resistance, it would be "an eccentric ellipsoid," that is, a figure resembling an ellipse. But though he had made out the approximate form of the curve, in some unexplained way, we have no reason to believe that he possessed any means of determining the mathematical properties of the curve described in such a case. The perpetual composition of a central force with the previous motion of the body, could not be successfully treated without the consideration of Limits, or something equivalent to them. The first example which we have of the right solution of such a problem occurs, so far as I know, in the theorems of Huyghens concerning circular motion, which were published, without demonstration, at the end of his *Horologium Oscillatorium*, in 1673. It was there asserted that when equal bodies describe circles, if the times are equal, the centrifugal forces will be as the diameters of the circles; if the velocities are equal, the forces will be reciprocally as the diameters, and so on. In order to arrive at these propositions, Huyghens must, virtually at least, have applied the second law of motion to the limiting elements of the curve, according to the way in which Newton, a few years later, gave the demonstration of the theorems of Huyghens in the Principia.

The growing persuasion that the motions of the heavenly bodies about the sun might be explained by the action of central forces, gave a peculiar interest to these mechanical speculations, at the

period now under review. Indeed, it is not easy to state separately, as our present object requires us to do, the progress of mechanics and the progress of astronomy. Yet the distinction which we have to make is, in its nature, sufficiently marked. It is, in fact, no less marked than the distinction between speaking logically and speaking truly. The framers of the science of motion were employed in establishing those notions, names, and rules, in conformity to which *all* mechanical *truth must* be expressed; but *what was the truth* with regard to the mechanism of the universe remained to be determined by other means. Physical astronomy, at the period of which we speak, eclipsed and overlaid theoretical mechanics, as, a little previously, dynamics had eclipsed and superseded statics.

The laws of variable force and of curvilinear motion were not much pursued, till the invention of Fluxions and of the Differential Calculus again turned men's minds to these subjects, as easy and interesting exercises of the powers of these new methods. Newton's Principia, of which the two first books are purely dynamical, is the great exception to this assertion; inasmuch as it contains correct solutions of a great variety of the most general problems of the science; and, indeed, is, even yet, one of the most complete treatises which we possess upon the subject.

We have seen that Kepler, in his attempts to explain the curvilinear motions of the planets by

means of a central force, failed, in consequence of his belief that a continued transverse action of the central body was requisite to keep up a continued motion. Galileo had founded his theory of projectiles on the principle that such an action was not necessary; yet Borelli, a pupil of Galileo, when, in 1666, he published his theory of the Medicean stars (the satellites of Jupiter), did not keep quite clear of the same errors which had vitiated Kepler's reasonings. In the same way, though Descartes is sometimes spoken of as the first promulgator of the first law of motion, yet his theory of vortices must have been mainly suggested by a want of an entire confidence in that law. When he represented the planets and satellites as owing their motions to oceans of fluid diffused through the celestial spaces, and constantly whirling round the central bodies, he must have felt afraid of trusting the planets to the operation of the laws of motion in free space. Sounder physical philosophers, however, began to perceive the real nature of the question. As early as 1666, we read, in the Journals of the Royal Society, that "there was read a paper of Mr. Hooke's, explicating the the inflexion of a direct motion into a curve by a supervening attractive principle;" and before the publication of the Principia in 1687, Huyghens, as we have seen, and, in our own country, Wren, Halley, and Hooke, had made some progress in the true mechanics of circular motion[2], and had distinctly

[2] Newt. Princip. Schol. to Prop. iv.

contemplated the problem of the motion of a body in an ellipse by a central force, though they could not solve it. Halley went to Cambridge in 1684[3], for the express purpose of consulting Newton upon the subject of the production of the elliptical motion of the planets by means of a central force, and, on the 10th of December[4], announced to the Royal Society that he had seen Mr. Newton's book, *De Motu Corporum*. The feeling that mathematicians were on the brink of discoveries such as are contained in this work was so strong, that Dr. Halley was requested to remind Mr. Newton of his promise of entering them in the Register of the Society, "for securing the invention to himself till such time as he can be at leisure to publish it." The manuscript, with the title "Philosophiæ Naturalis Principia Mathematica," was presented to the society (to which it was dedicated,) on the 28th of April, 1686. Dr. Vincent, who presented it, spoke of the novelty and dignity of the subject; and the president (Sir J. Hoskins) added, with great truth, "that the method was so much the more to be prized as it was both invented and perfected at the same time."

The reader will recollect that we are here speaking of the Principia as a mechanical treatise only; we shall afterwards have to consider it as containing the greatest discoveries of physical astronomy. As a work on dynamics, its merit is, that it contains a

[3] Brewster's Life of Newt. p. 154. [4] Br. N. p. 184.

wonderful store of refined and beautiful mathematical artifices, applied to solve all the most general problems which the subject offered. It can hardly be said to contain any new inductive discovery respecting the principles of mechanics; for though Newton's "Axioms or Laws of Motion," which stand at the beginning of the book, are a much clearer and more general statement of the grounds of mechanics than had yet appeared, it can hardly be said that they contain any doctrines which had not been previously stated or taken for granted by other mathematicians.

The work, however, besides its unrivalled mathematical skill, employed in tracing out, deductively, the consequences of the laws of motion, and its great cosmical discoveries, which we shall hereafter treat of, had great philosophical value in the history of Dynamics, as exhibiting a clear conception of the new character and functions of that science. In his preface, Newton says, "Rational mechanics must be the science of the motions which result from any forces, and of the forces which are required for any motions, accurately propounded and demonstrated. For many things induce me to suspect, that all natural phenomena may depend upon some forces by which the particles of bodies are either drawn towards each other, and cohere, or repel and recede from each other: and these forces being hitherto unknown, philosophers have pursued their researches in vain. And I hope that the principles expounded

in this work will afford some light, either to this mode of philosophizing, or to some mode which is more true."

Before we pursue this subject further, we must trace the remainder of the history of the third law.

Sect. 2.—Generalisation of the Third Law of Motion. Centre of Oscillation.—Huyghens.

THE third law of motion, whether expressed according to Newton's formula, by the equality of action and reaction, or in any other of the ways employed about the same time, easily gave the solution of mechanical problems in all cases of *direct* action; that is, when each body acted directly on others. But there still remained the problems in which the action is *indirect*;—when bodies, in motion, act on each other by the intervention of levers, or in any other way. If a rigid rod, passing through two weights, be made to swing about its upper point, so as to form a pendulum, each weight will act and react on the other by means of the rod, considered as a lever turning about the point of suspension. What, in this case, will be the effect of this action and reaction? In what time will the pendulum oscillate by the force of gravity? Where is the point at which a single weight must be placed to oscillate in the same time? in other words, where is the *Centre of Oscillation?*

Such was the problem, an example only of the general problem of indirect action, which mathema-

ticians had to solve. That it was by no means easy and obvious in what manner the law of the communication of motion was to be extended from simpler cases to those where rotatory motion was produced, is shown by this;—that Newton, in attempting to solve the mechanical problem of the precession of the equinoxes, fell into a serious error on this very subject. He assumed that, when a part has to communicate rotatory movement to the whole, (as the protuberant portion of the terrestrial spheroid, attracted by the sun and moon, communicates a small movement to the whole mass of the earth,) the quantity of the *motion*, "motus," will not be altered by being communicated. This principle is true, if, by *motion*, we understand what is called *moment of inertia*, a quantity in which both the velocity of each particle and its distance from the axis of rotation are taken into account: but Newton, in his calculations of its amount, considered the velocity only; thus making *motion*, in this case, identical with the *momentum* which he introduces in treating of the simpler case of the third law of motion, when the action is direct. This error was retained even in the later editions of the Principia[5].

The question of the centre of oscillation had been proposed by Mersenne somewhat earlier[6], in 1646. And though the problem was out of the reach of any principles at that time known and understood, some

[5] B. iii. Lemma iii. to Prop. xxxix. [6] Mont. ii. 423.

of the mathematicians of that time had rightly solved some cases of it, by proceeding as if the question had been to find the *Centre of Percussion*. The centre of percussion is the point about which the momenta of all the parts of a body balance each other, when it is in motion about any axis, and is stopped by striking against an obstacle placed at that centre. Roberval found this point in some easy cases; Descartes also attempted the problem; their rival labours led to an angry controversy: and Descartes was, as in his physical speculations he often was, very presumptuous, though not more than half right.

Huyghens was hardly advanced beyond boyhood when Mersenne first proposed this problem; and, as he says[7], could see no principle which even offered an opening to the solution, and had thus been repelled at the threshold. When, however, he published his "Horologium Oscillatorium" in 1673, the fourth part of that work was on the Centre of Oscillation or Agitation; and the principle which he then assumed, though not so simple and self-evident as those to which such problems were afterwards referred, was perfectly correct and general, and led to exact solutions in all cases. The reader has already seen repeatedly in the course of this history, complex and derivative principles presenting themselves to men's minds before simple and elementary ones. The "hypothesis" assumed by Huyghens was

[7] Hor. Osc. Prof.

this; "that if any weights are put in motion by the force of gravity, they *can not* move so that the centre of gravity of them all shall rise *higher* than the place from which it descended." This being assumed, it is easy to show that the centre of gravity will, under all circumstances, rise *as high* as its original position; and this consideration leads to a determination of the oscillation of a compound pendulum. We may observe, in the principle thus selected, a conviction that, in all mechanical action, the centre of gravity may be taken as the representative of the whole system. This conviction, as we have seen, may be traced in the axioms of Archimedes; and Huyghens, when he proceeds upon it, undertakes to show[8], that he assumes only this, that a heavy body cannot, of itself, move upwards.

Clear as Huyghens's principle appeared to himself, it was, after some time, attacked by the Abbé Catelan, a zealous Cartesian. Catelan also put forth principles which he conceived were evident, and deduced from them conclusions contradictory to those of Huyghens. His principles, now that we know them to be false, appear to us very gratuitous. They are these; "that in a compound pendulum, the sum of the velocities of the component weights is equal to the sum of the velocities which they would have acquired if they had been detached pendulums;" and "that the time of the vibration of a compound

[8] Hor. Osc. p. 121.

pendulum is an arithmetic mean between the times of the vibrations of the weights, moving as detached pendulums." Huyghens easily showed that these suppositions would make the centre of gravity ascend to a greater height than that from which it fell; and after some time, James Bernoulli stept into the arena, and ranged himself on the side of Huyghens. As the discussion thus proceeded, it began to be seen that the question really was, in what manner the third law of motion was to be extended to cases of indirect action; whether by distributing the action and re-action according to statical principles, or in some other way. "I propose it to the consideration of mathematicians," says Bernoulli in 1686, "what law of the communication of velocity is observed by bodies in motion, which are sustained at one extremity by a fixed fulcrum, and at the other by a body also moving, but more slowly. Is the excess of velocity which must be communicated from the one body to the other to be distributed in the same proportion in which a load supported on the lever would be distributed?" He adds, that if this question be answered in the affirmative, Huyghens will be found to be in error; but this is a mistake. The principle, that the action and re-action of bodies thus moving are to be distributed according to the rules of the lever, is true; but Bernoulli mistook, in estimating this action and re-action by the *velocity* acquired at any moment; instead of taking, as he should have done, the *increment* of

velocity which gravity tended to impress in the next instant. This was shown by the Marquis de l'Hôpital; who adds, with justice, "I conceive that I have thus fully answered the call of Bernoulli, when he says, I propose it to the consideration of mathematicians, &c."

We may, from this time, consider as known, but not as fully established, the principle that "when bodies in motion affect each other, the action and reaction are distributed according to the laws of statics;" although there were still found occasional difficulties in the generalisation and application of the rule. James Bernoulli, in 1703, gave a "general demonstration of the centre of oscillation, drawn from the nature of the lever." In this demonstration[9] he takes as a fundamental principle, that bodies in motion, connected by levers, balance, when the products of their momenta and the lengths of the levers are equal in opposite directions. For the proof of this proposition, he refers to Marriotte, who had asserted it of weights acting by percussion[10], and in order to prove it, had balanced the effect of a weight on a lever by the effect of a jet of water, and had confirmed it by other experiments[11]. Moreover, says Bernoulli, there is no one who denies it. Still, this kind of proof was hardly satisfactory or elementary enough. John Bernoulli took up the subject after the death of his brother James, which happened in 1705.

[9] Op. ii. 930. [10] Choq. des Corps. p. 296. [11] Ib. Prop. xi.

The former published in 1714 his "Meditatio de naturâ Centri Oscillationis." In this memoir, he assumes, as his brother had done, that the effects of forces on a lever in motion are distributed according to the common rules of the lever[12]. The principal generalisation which he introduced was, that he considered gravity as a force soliciting to motion, which might have different intensities in different bodies. At the same time, Brook Taylor in England solved the problem, upon the same principles as Bernoulli; and the question of priority on this subject was one point in the angry intercourse which, about this time, became common between the English mathematicians and those of the continent. Hermann also, in his "Phoronomia," published in 1716, gave a proof which, as he informs us, he had devised before he saw John Bernoulli's. This proof is founded on the statical equivalence of the "*solicitations of gravity*" and the "*vicarious solicitations*" which correspond to the actual motion of each part; or, as it has been expressed by more modern writers, the equilibrium of the *impressed* and *effective forces*.

It was shown by John Bernoulli and Hermann, and was indeed easily proved, that the proposition assumed by Huyghens as the foundation of his solution, was, in fact, a consequence of the elementary principles which belong to this branch of mechanics. But this assumption of Huyghens was an example

[12] p. 172.

of a more general proposition, which by some mathematicians at this time had been put forward as an original and elementary law;—a principle which ought to supersede the usual measure of the forces of bodies in motion; this principle they called "*the conservation of vis viva.*" The attempt to make this change was the commencement of one of the most obstinate and curious of the controversies which form part of the history of mechanical science. The celebrated Leibnitz was the author of the new opinion. In 1686, he published, in the Leipsic Acts, "A short Demonstration of a memorable error of Descartes and others, concerning the natural law by which they think that God always preserves the same quantity of motion; in which they pervert mechanics." The principle that the same quantity of motion, and therefore of moving force, is always preserved in the world, follows from the equality of action and reaction; though Descartes had, after his fashion, given a theological reason for it; Leibnitz allowed that the quantity of moving force remains always the same, but denied that this force is measured by the quantity of motion or momentum. He maintained that the same force is requisite to raise a weight of one pound through four feet, and a weight of four pounds through one foot, though the momenta in this case are as one to two. This was answered by the Abbé de Conti; who truly observed, that allowing the effects in the two cases to be equal, this did not prove the forces to be equal; since the effect, in the

first case, was produced in a double time, and therefore it was quite consistent to suppose the force only half as great. Leibnitz, however, persisted in his innovation; and in 1695 laid down the distinction between *vires mortuæ*, or pressures, and *vires vivæ*, the name he gave to his own measure of force. He kept up a correspondence with John Bernoulli, whom he converted to his peculiar opinions on this subject; or rather, as Bernoulli says[13], made him think for himself, which ended in his proving directly that which Leibnitz had defended by indirect reasons. Among other arguments, he had pretended to show (what is certainly not true,) that if the common measure of forces be adhered to, a perpetual motion would be possible. It is easy to collect many cases which admit of being very simply and conveniently reasoned upon by means of the vis viva, that is, by taking the force to be proportional to the square of the velocity, and not to the velocity itself. Thus, in order to give the arrow twice the velocity, the bow must be four times as strong; and in all cases in which no account is taken of the time of producing the effect, we may conveniently use similar methods.

But it was not till a later period that the question excited any general notice. The Academy of Sciences of Paris in 1724 proposed as a subject for their prize dissertation the laws of the impact of bodies. Bernoulli, as a competitor, wrote a treatise, upon Leib-

[13] Op. iii. 40.

nitzian principles, which, though not honoured with the prize, was printed by the Academy with commendation[14]. The opinions which he here defended and illustrated were adopted by several mathematicians; the controversy extended from the mathematical to the literary world, at that time more attentive than usual to mathematical disputes, in consequence of the great struggle then going on between the Cartesian and the Newtonian system. It was, however, obvious, that by this time the interest of the question, so far as the progress of dynamics was concerned, was at an end; for the combatants all agreed as to the results in each particular case. The laws of motion were now established; and the question was, by means of what definitions and abstractions could they be best expressed;—a metaphysical, not a physical discussion, and therefore one in which "the paper philosophers," as Galileo called them, could bear a part. In the first volume of the Transactions of the Academy of St. Petersburgh, published in 1728, there are three Leibnitzian memoirs by Hermann, Bullfinger, and Wolff. In England, Clarke was an angry assailant of the German opinion, which S'Gravesande maintained. In France, Mairan attacked the *vis viva* in 1728; "with strong and victorious reasons," as the Marquise du Chatelet declared, in the first edition of her *Treatise on Fire*[15]. But shortly after this praise was published, the

[14] Discours sur les Loix de la Communication du Mouvement.

[15] Mont. iii. 640.

Chateau de Cirey, where the Marquise usually lived, became a school of Leibnitzian opinions, and the resort of the principal partisans of the *vis viva*. "Soon," observes Mairan, "their language was changed; the *vis viva* was enthroned by the side of the *monads*." The Marquise tried to retract or explain away her praises; she urged arguments on the other side. Still the question was not decided; even her friend Voltaire was not converted. In 1741 he read a memoir "On the Measure and Nature of Moving Forces," in which he maintained the old opinion. Finally, D'Alembert in 1743 declared it to be, as it truly was, a mere question of words; and by the turn which dynamics then took, it ceased to be of any possible interest or importance to mathematicians.

The representation of the laws of motion and of the reasonings depending on them, in the most general form, by means of analytical language, cannot be said to have been fully achieved till the time of D'Alembert; but as we have already seen, the discovery of these laws had taken place somewhat earlier; and that law which is more particularly expressed in D'Alembert's principle (*the equality of the action gained and lost*) was, it has been seen, rather led to by the general current of the reasoning of mathematicians about the end of the seventeenth century than discovered by any one. Huyghens, Marriotte, the two Bernoullis, L'Hôpital, Taylor, and Hermann, have each of them their name in the history of this

advance; but we cannot ascribe to any of them any great real inductive sagacity shown in what they thus contributed, except to Huyghens, who first seized the principle in such a form as to find the centre of oscillation by means of it. Indeed, in the steps taken by the others, language itself had almost made the generalisation for them at the time when they wrote; and it required no small degree of acuteness and care to distinguish the old cases, in which the law had already been applied, from the new cases, in which they had to apply it.

CHAPTER VI.

Sequel of the Generalisation of the Principles of Mechanics.—Period of Mathematical Deduction.—Analytical Mechanics.

We have now finished the history of the discovery of mechanical principles, strictly so called. The three laws of motion, generalised in the manner we have described, contain the materials of the whole structure of mechanics; and in the remaining progress of the science, we are led to no new truth which was not implicitly involved in those previously known. It may be thought, therefore, that the narrative of this progress is of comparatively small interest. Nor do we maintain that the application and developement of principles is a matter of so much importance to the philosophy of science, as the advance towards and to them. Still, there are many circumstances in the latter stages of the progress of the science of mechanics, which well deserve notice; and make a rapid survey of that part of its history indispensable to our purpose.

The laws of motion are expressed in terms of space and number; the developement of the consequences of these laws must, therefore, be performed by means of the reasonings of mathematics; and the

science of mechanics may assume the various aspects which belong to the different modes of dealing with mathematical quantities. Mechanics may, like pure mathematics, be geometrical or analytical; that is, it may treat space by a direct consideration of its properties, or by a symbolical representation of them: mechanics, like pure mathematics, may proceed from special cases, to problems and methods of extreme generality;—may summon to its aid the curious and refined relations of symmetry, by which general and complex conditions are simplified;—may become more powerful by the discovery of more powerful analytical artifices;—may even have the generality of its principles further expanded, inasmuch as symbols are a more general language than words. We shall very briefly notice a series of modifications of this kind.

1. *Geometrical Mechanics. Newton, &c.*—The first great systematical Treatise on Mechanics, in the most general sense, is the two first Books of the *Principia* of Newton. In this work, the method employed is predominantly geometrical: not only space is not represented symbolically, or by reference to number; but numbers, as, for instance, those which measure time and force, are represented by spaces; and the laws of their changes are indicated by the properties of curve lines. It is well known that Newton employed, by preference, methods of this kind in the exposition of his theorems, even where he had made the discovery of them by analytical calculations.

The intuitions of space appeared to him, as they have appeared to many of his followers, to be a more clear and satisfactory road to knowledge, than the operations of symbolical language. Hermann, whose *Phoronomia* was the next great work on this subject, pursued a like course; employing curves, which he calls "the scale of velocities," "of forces," &c. Methods nearly similar were employed by the two first Bernoullis, and other mathematicians of that period; and were, indeed, so long familiar, that the influence of them may still be traced in some of the terms which are used on such subjects; as, for instance, when we talk of "reducing a problem to quadratures," that is, to the finding the area of the curves employed in these methods.

2. *Analytical Mechanics. Euler.*—As analysis was more cultivated, it gained a predominancy over geometry; being found to be a far more powerful instrument for obtaining results; and possessing a beauty and an evidence, which, though different from those of geometry, had great attractions for minds to which they became familiar. The person who did most to give to analysis the generality and symmetry which are now its pride, was also the person who made mechanics analytical; I mean Euler. He began his execution of this task in various memoirs which appeared in the Transactions of the Academy of Sciences at St. Petersburgh, commencing with its earliest volumes; and in 1736, he published there his "Mechanics, or the Science of Motion analytically

expounded; in the way of a Supplement to the Transactions of the Imperial Academy of Sciences." In the preface to this work, he says, that though the solutions of problems by Newton and Hermann were quite satisfactory, yet he found that he had a difficulty in applying them to new problems, differing little from theirs; and that, therefore, he thought it would be useful to extract an analysis out of their synthesis.

3. *Mechanical Problems.*—In reality, however, Euler has done much more than merely give analytical methods, which may be applied to mechanical problems: he has himself applied such methods to an immense number of cases. His transcendent mathematical powers, his long and studious life, and the interest with which he pursued the subject, led him to solve an almost inconceivable number and variety of mechanical problems. Such problems suggested themselves to him on all occasions. One of his memoirs begins, by stating that, happening to think of the line of Virgil,

> Anchora de prorâ jacitur stant litore puppes;
>
> The anchor drops, the rushing keel is staid;

he could not help inquiring what would be the nature of the ship's motion under the circumstances here described. And in the last few days of his life, after his mortal illness had begun, having seen in the newspapers some statements respecting balloons, he proceeded to calculate their motions; and performed

a difficult integration, in which this undertaking engaged him. His memoirs occupy a very large portion of the Petropolitan Transactions during his life, from 1728 to 1783; and he declared that he should leave papers which might enrich the publications of the Academy of Petersburg for twenty years after his death;—a promise which has been more than fulfilled; for, up to 1818, the volumes usually contain several memoirs of his. He and his contemporaries may be said to have exhausted the subject; for there are few mechanical problems which have been since treated, which they have not in some manner touched upon.

I do not dwell upon the details of such problems; for the next great step in analytical mechanics, the publication of D'Alembert's principle in 1743, in a great degree superseded their interest. The Transactions of the Academies of Paris and Berlin, as well as St. Petersburg, are filled, up to this time, with various questions of this kind. They require, for the most part, the determination of the motions of several bodies, with or without weight, which pull or push each other by means of threads, or levers, to which they are fastened, or along which they can slide; and which, having a certain impulse given them at first, are then left to themselves, or are compelled to move in given lines and surfaces. The postulate of Huyghens, respecting the motion of the centre of gravity, was generally one of the principles of the solution; but other principles were always needed in addition to

this; and it required the exercise of ingenuity and skill to detect the most suitable in each case. Such problems were, for some time, a sort of trial of strength among mathematicians: the principle of D'Alembert put an end to this kind of challenges, by supplying a direct and general method of resolving, or at least of throwing into equations, any imaginable problem. The mechanical difficulties were in this way reduced to difficulties of pure mathematics.

4. *D'Alembert's Principle.*—D'Alembert's principle is only the expression, in the most general form, of the principle upon which John Bernoulli, Hermann, and others, had solved the problem of the centre of oscillation. It was thus stated, "The motion *impressed* on each particle of any system by the forces which act upon it, may be resolved into two, the *effective* motion, and the motion gained and *lost:* the effective motions will be the real motions of the parts, and the motions gained and lost will be such as would keep the system at rest." The distinction of *statics*, the doctrine of equilibrium, and *dynamics*, the doctrine of motion, was, as we have seen, fundamental; and the difference of difficulty and complexity in the two subjects was well understood, and generally recognised by mathematicians. D'Alembert's principle reduces every dynamical question to a statical one; and hence, by means of the conditions which connect the possible motions of the system, we can determine what the actual motions must be. The difficulty of determining the laws of *equilibrium, in the* application of this principle in

complex cases, is, however, often as great as if we apply more simple and direct considerations.

5. *Motion in Resisting Media. Ballistics.*—We shall notice more particularly the history of some of the problems of mechanics. Though John Bernoulli always spoke with admiration of the Principia, and of its author, he appears to have been well disposed to point out real or imagined blemishes in the work. Against the validity of Newton's determination of the path described by a body projected in any part of the solar system, Bernoulli urges a cavil which it is difficult to conceive that a mathematician, such as he was, could seriously believe to be well founded. On Newton's determination of the path of a body in a resisting medium, his criticism is more just. He pointed out a material error in this solution: this correction came to Newton's knowledge in London, in October 1712, when the impression of the second edition of the Principia was just drawing to a close, under the care of Cotes at Cambridge: and Newton immediately cancelled the leaf and corrected the error[1].

This problem of the motion of the body in a resisting medium, led to another collision between the English and the German mathematicians. The proposition to which we have referred, gave only an indirect view of the nature of the curve described by a projectile in the air; and it is probable that Newton,

[1] M.S. Correspondence in Trin. Coll. Library.

when he wrote the Principia, did not see his way to any direct and complete solution of this problem. At a later period, in 1718, when the quarrel had waxed hot between the admirers of Newton and Leibnitz, Keill, who had come forward as a champion on the English side, proposed this problem to the foreigners as a challenge. Keill probably imagined that what Newton had not discovered, no one of his time would be able to discover. But the sedulous cultivation of analysis by the Germans had given them mathematical powers beyond the expectation of the English; who, whatever might be their talents, had made little advance in the effective use of general methods; and for a long period seemed to be fascinated to the spot, in their admiration of Newton's excellence. Bernoulli speedily solved the problem; and reasonably enough, according to the law of honour of such challenges, called upon the challenger to produce his solution. Keill was unable to do this; and after some attempts at procrastination, was driven to very paltry evasions. Bernoulli then published his solution, with very just expressions of scorn towards his antagonist. And this may, perhaps, be considered as the first material addition which was made to the Principia by subsequent writers.

6. *Constellation of Mathematicians.*—We pass with admiration along the great series of mathematicians, by whom the science of theoretical mechanics has been cultivated, from the time of Newton to our

own. There is no group of men of science whose fame is higher or brighter. The great discoveries of Copernicus, Galileo, Newton, had fixed all eyes on those portions of human knowledge on which their successors employed their labours. The certainty belonging to this line of speculation seemed to elevate mathematicians above the students of other subjects; and the beauty of mathematical relations, and the subtlety of intellect which may be shown in dealing with them, were fitted to win unbounded applause. The successors of Newton and the Bernoullis, as Euler, Clairaut, D'Alembert, Lagrange, Laplace, not to introduce living names, have been some of the most remarkable men of talent which the world has seen. That their talent is, for the most part, of a different kind from that by which the laws of nature were discovered, I shall have occasion to explain elsewhere; for the present, I must endeavour to arrange the principal achievements of those whom I have mentioned.

The series of persons is connected by social relations. Euler was the pupil of the first generation of Bernoullis, and the intimate friend of the second generation; and all these extraordinary men, as well as Hermann, were of the city of Basil, in that age a spot fertile of great mathematicians to an unparalleled degree. In 1740, Clairaut and Maupertuis visited John Bernoulli, at that time the Nestor of mathematicians, who died, full of age and honours, in 1748. Euler, several of the Bernoullis, Maupertuis,

Lagrange, among other mathematicians of smaller note, were called into the north by Catherine of Russia and Frederic of Prussia, to inspire and instruct academies which the brilliant fame then attached to science, had induced those monarchs to establish. The prizes proposed by these societies, and by the French Academy of Sciences, gave occasion to many of the most valuable mathematical works of the century.

7. *The Problem of three Bodies.*—In 1747, Clairaut and D'Alembert sent, on the same day, to this body, their solutions of the celebrated "problem of three bodies," which, from that time, became the great object of attention of mathematicians;—the bow in which each tried his strength, and endeavoured to shoot further than his predecessors.

This problem was, in fact, the astronomical question of the effect produced by the attraction of the sun, in disturbing the motions of the moon about the earth; or by the attraction of one planet, disturbing the motion of another planet about the sun; but being expressed generally, as referring to one body which disturbs any two others, it became a mechanical problem, and the history of it belongs to the present subject.

One consequence of the synthetical form adopted by Newton in the Principia, was, that his successors had the problem of the solar system to begin entirely anew. Those who would not do this, made no progress, as was long the case with the English.

Clairaut says, that he tried for a long time to make some use of Newton's labours; but that, at last, he resolved to take up the subject in an independent manner. This, accordingly, he did, using analysis throughout, and following methods not much different from those still employed. We do not now speak of the comparison of this theory with observation, except to remark, that both by the agreements and by the discrepancies of this comparison, Clairaut and other writers were perpetually driven on to carry forwards the calculation to a greater and greater degree of accuracy.

One of the most important of the cases in which this happened, was that of the movement of the apogee of the moon; and in this case a mode of approximating to the truth, which had been depended on as nearly exact, was, after having caused great perplexity, found by Clairaut and Euler to give only half the truth. This same problem of three bodies was the occasion of a memoir of Clairaut, which gained the prize of the Academy of St. Petersburg in 1751; and, finally, of his *Théorie de la Lune*, published in 1765. D'Alembert laboured at the same time on the same problem; and the value of their methods, and the merit of the inventors, unhappily became a subject of controversy between those two great mathematicians. Euler also, in 1753, published a *Theory of the Moon*, which was, perhaps, more useful than either of the others, since it was afterwards the basis of Mayer's method, and of his tables

It is difficult to give the general reader any distinct notion of these solutions. We may observe, that the quantities which determine the moon's position, are to be determined by means of certain algebraical equations, which express the mechanical conditions of the motion. The operation, by which the result is to be obtained, involves the process of integration; which, in this instance, cannot be performed in an immediate and definite manner; since the quantities thus to be operated on depend on the moon's position, and thus require us to know the very thing which we have to determine by the operation. The result must be got at, therefore, by successive approximations: we must first find a quantity near the truth; and then, by the help of this, one nearer still; and so on; and, in this manner, the moon's place will be given by a converging series of terms. The form of these terms depends upon the relations of position between the sun and moon, their apogees, the moon's nodes, and other quantities; and by the variety of combinations of which these admit, the terms become very numerous and complex. The magnitude of the terms depends also upon various circumstances; as the relative force of the sun and earth, the relative times of the solar and lunar revolutions, the eccentricities and inclinations of the two orbits. These are combined so as to give terms of different orders of magnitudes; and it depends upon the skill and perseverance of the mathematician how far he will continue this series of terms. For there

is no limit to their number; and though the methods of which we have spoken do theoretically enable us to calculate as many terms as we please, the labour and the complexity of the operations are so serious that common calculators are stopped by them. None but very great mathematicians have been able to walk safely any considerable distance into this avenue, —so rapidly does it darken as we proceed. And even the possibility of doing what has been done, depends upon what we may call accidental circumstances; the smallness of the inclinations and eccentricities of the system, and the like. "If nature had not favoured us in this way," Lagrange used to say, "there would have been an end of the geometry in this problem." The expected return of the comet of 1682 in 1759, gave a new interest to the problem, and Clairaut proceeded to calculate the case which was thus suggested. When this was treated by the methods which had succeeded for the moon, it offered no prospect of success, in consequence of the absence of the favourable circumstances just referred to, and, accordingly, Clairaut[1], after obtaining the six equations to which he reduces the solution, adds, "Integrate them who can;" (Intégre maintenant qui pourra). New methods of approximation were devised for this case.

The problem of three bodies was not prosecuted in consequence of its analytical beauty, or its intrinsic attraction; but its great difficulties were thus

[1] Journal des Sçavans, Aug. 1759.

resolutely combated from necessity; because in no other way could the theory of universal gravitation be known to be true or made to be useful. The construction of *Tables of the Moon*, an object which offered a large pecuniary reward, as well as well as mathematical glory, to the successful adventurer, was the main purpose of these labours.

The *Theory of the Planets* presented the problem of three bodies in a new form, and involved in peculiar difficulties; for the approximations which succeed in the lunar theory fail here. Artifices somewhat modified are required to overcome the difficulties of this case.

Euler had investigated, in particular, the motions of Jupiter and Saturn, in which there was a secular acceleration and retardation, known by observation, but not easily explicable by theory. Euler's memoirs, which gained the prize of the French Academy, in 1748 and 1752, contained much beautiful analysis; and Lagrange published also a theory of Jupiter and Saturn, in which he obtained results different from those of Euler. Laplace, in 1787, showed that this inequality arose from the circumstance that two of Saturn's years are very nearly equal to five of Jupiter's.

The problems relating to Jupiter's *Satellites*, were found to be even more complex than those which refer to the planets: for it was necessary to consider each satellite as disturbed by the other three at once; and thus there occurred the problem

of *five* bodies. This problem was resolved by Lagrange[3].

Again, the newly-discovered *small Planets*, Juno, Ceres, Vesta, Pallas, whose orbits almost coincide with each other, and are more inclined and more eccentric than those of the ancient planets, give rise, by their perturbations, to new forms of the problem, and require new artifices.

In the course of these researches respecting Jupiter, Lagrange and Laplace were led to consider particularly the *secular inequalities* of the solar system; that is, those inequalities in which the duration of the cycle of change embraces very many revolutions of the bodies themselves. Lagrange[4], in 1766, had introduced the method of the *variation of the elements* of the orbit; which consists in tracing the effect of the perturbing forces, not as directly altering the place of the planet, but as producing a change from one instant to another, in the dimensions and position of the orbit which the planet describes. Taking this view, he determines the secular changes of each of the *elements*, or determining quantities of the orbit. In 1773, Laplace also attacked this subject of secular changes, and obtained expressions for them. On this occasion, he proved the celebrated proposition that, "the mean motions of the planets are invariable:" that is, that there is, in the revolutions of the system, no progressive change which is not finally

[3] Bailly. Ast. Mod. iii. 178. [4] Gautier. Prob. de Trois Corps. p. 155.

stopped and reversed; no increase, which is not, after some period, changed into decrease; no retardation which is not at last succeeded by acceleration; although, as we have seen, millions of years may elapse before the system reaches the turning point. Thomas Simpson noticed the same consequence of the laws of universal attraction. In 1774 and 1776 Lagrange[5] still laboured at the secular equations; extending his researches to the nodes and inclinations; and showed that the invariability of the mean motions of the planets, which Laplace had proved, neglecting the fourth powers of the eccentricities and inclinations of the orbits[6], was true, however far the approximation was carried, so long as the squares of the disturbing masses were neglected. He afterwards improved his methods[7]; and, in 1783, he endeavoured to extend the calculation of the changes of the elements to the periodical equations, as well as the secular.

8. *Mécanique Céleste, &c.*—Laplace also resumed the consideration of the secular changes; and, finally, undertook his vast work, the *Mécanique Céleste*, which he intended to contain a complete view of the existing state of this splendid department of science We may see, in the exultation which the author obviously feels at the thought of erecting this monument of his age, the enthusiasm which had been excited by the splendid course of mathematical

[5] Gautier, p. 104. [6] Ib. p. 184. [7] Ib. p. 196.

successes of which I have given a sketch. The two first volumes of this great work appeared in 1799. The third and fourth volumes were published in 1802 and 1805 respectively. Since its publication, little has been added to the solution of the great problems of which it treats. In 1808, Laplace presented to the French Bureau des Longitudes, a Supplement to the Mécanique Céleste; the object of which was to improve still further the mode of obtaining the secular variations of the elements. Poisson and Lagrange proved the invariability of the major axes of the orbits, as far as the second order of the perturbing forces. Various other authors have since laboured at this subject. Burckhardt, in 1808, extended the perturbing function as far as the sixth order of the eccentricities. Gauss, Hansen, and Bessel, Ivory, Lubbock, Pontécoulant, and Airy, have, at different periods up to the present time, either extended or illustrated some particular part of the theory, or applied it to special cases; as in the instance of Professor Airy's calculation of an inequality of Venus and the earth, of which the period is 240 years. And, finally, Plana has once more attempted to present, in a single work (two thick quarto volumes), all that has hitherto been executed with regard to the theory of the moon.

I give only the leading points of the progress of analytical dynamics. Hence I have not spoken in detail of the theory of the satellites of Jupiter, a subject on which Lagrange gained a prize for a

Memoir, in 1766, and in which Laplace discovered some most curious properties in 1784. Still less have I referred to the purely speculative question of *Tautochronous Curves* in a resisting medium, though it was a subject of the labours of Bernoulli, Euler, Fontaine, D'Alembert, Lagrange, and Laplace. The reader will suppose that many other curious investigations are passed over in utter silence.

9. *Precession. Motion of Rigid Bodies.* — The series of investigations of which I have spoken, extensive and complex as it is, treats the moving bodies as points only, and takes no account of any peculiarity of their form or motion of their parts. The investigation of the motion of a body of any magnitude and form, is another branch of analytical mechanics, which well deserves notice. Like the former branch, it mainly owed its cultivation to the problems suggested by the solar system. Newton, as we have seen, endeavoured to calculate the effect of the attraction of the sun and moon in producing the *precession of the equinoxes;* but in doing this he made some mistakes. In 1747, D'Alembert solved this problem by the aid of his "principle;" and it was not difficult for him to show, as he did in his "Opuscules," in 1761, that the same method enabled him to determine the motion of a body of any figure acted upon by any forces. But, as the reader will have observed in the course of this narrative, the great mathematicians of this period were always nearly abreast of each other in their advances.—

Euler[8], in the mean time, had published, in 1751, a solution of the problem of the precession; and in 1752, a memoir which he entitled, "Discovery of a New Principle of Mechanics," and which contains a solution of the general problem of the alteration of rotary motion by forces. D'Alembert noticed with disapprobation the assumption of priority which this title implied, though allowing the merit of the memoir. Various improvements were made in these solutions; but the final form was given them by Euler; and they were applied to a great variety of problems in his "Theory of the Motion of Solid and Rigid Bodies," which was written[9] about 1760, and published in 1765. The formulæ in this work were much simplified by the use of a discovery of Segner, that every body had three axes which were called principal axes, about which alone (in general) it would permanently revolve. The equations which Euler and other writers had obtained, were attacked as erroneous by Landen in the Philosophical Transactions for 1785; but I think it is impossible to consider this criticism otherwise than as an example of the inability of the English mathematicians of that period to take a steady hold of the analytical generalisations to which the great continental authors had been led. Perhaps one of the most remarkable calculations of the motion of a rigid body is that which Lagrange performed with regard to the *moon's libra-*

[8] Ac. Berl. 1745, 1750. [9] See the preface to the book.

tion; and by which he showed that the nodes of the moon's equator and those of her orbit must always coincide.

10. *Vibrating Strings.*—Other mechanical questions, unconnected with astronomy, were also pursued with great zeal and success. Among these was the problem of a vibrating string, stretched between two fixed points. There is not much complexity in the mechanical conceptions which belong to this case, but considerable difficulty in reducing them to analysis. Taylor, in his "Method of Increments," published in 1716, had annexed a solution of this problem; obtained on suppositions, limited indeed, but apparently conformable to the most common circumstances of practice. John Bernoulli, in 1728, had also treated it. But the problem assumed an interest altogether new, when, in 1747, D'Alembert published his views on the subject; in which he maintained that, instead of one kind of curve only, there were an infinite number of different curves, which answered the conditions of the question. The problem, thus put forward by one great mathematician, was, as usual, taken up by the others, whose names the reader is now so familiar with in such an association. In 1748, Euler not only assented to the generalisation of D'Alembert, but held that it was not necessary that the curves so introduced should be defined by any algebraical condition whatever. From this extreme indeterminateness D'Alembert dissented; while Daniel Bernoulli,

trusting more to physical and less to analytical reasonings, maintained that both these generalisations were inapplicable in fact, and that the solution was really restricted, as had at first been supposed, to the form of the trochoid, and to other forms derivable from that. He introduced, in such problems, the "law of coexistent vibrations," which is of eminent use in enabling us to conceive the results of complex mechanical conditions, and the real import of many analytical expressions. In the mean time, the wonderful analytical genius of Lagrange had applied itself to this problem. He had formed the Academy of Turin, in conjunction with his friends Saluces and Cigna; and the first memoir in their Transactions was one by him on this subject: in this and in subsequent writings he has established, to the satisfaction of the mathematical world, that the functions introduced in such cases are not necessarily continuous, but are arbitrary to the same degree that the motion is so practically; though capable of expression by a series of circular functions. This controversy, concerning the degree of lawlessness with which the conditions of the solution may be assumed, is of consequence, not only with respect to vibrating strings, but also with respect to many problems, belonging to a branch of mechanics which we now have to mention, the doctrine of fluids.

11. *Equilibrium of Fluids.—Figure of the Earth.—Tides.*—The application of the general doctrines of mechanics to fluids was a natural and inevitable step,

when the principles of the science had been generalised. It was easily seen that a fluid is, for this purpose, nothing more than a body of which the parts are moveable amongst each other with entire facility; and that the mathematician must trace the consequences of this condition upon his equations. This accordingly was done, by the founders of mechanics, both for the cases of the equilibrium and of motion. Newton's attempt to solve the problem of the *figure of the earth*, supposing it fluid, is the first example of such an investigation, and this rested upon principles which we have already explained, applied with the skill and sagacity which distinguished all that Newton did.

We have already seen how the generality of the principle, that fluids press equally in all directions, was established. In applying it to calculation, Newton took for his fundamental principle, the equal weight of columns of the fluid reaching to the centre; Huyghens took, as his basis, the perpendicularity of the resulting force at each point to the surface of the fluid; Bouguer conceived that both principles were necessary; but Clairaut showed that the equilibrium of all canals is requisite. He also was the first mathematician who deduced from this principle the equations of partial differentials by which these laws are expressed; a step which, as Lagrange says[10], changed the face of hydrostatics,

[10] Méc. Analyt. ii. p. 180.

and made it a new science. Euler simplified the mode of obtaining the equations of equilibrium for any forces whatever; and put them in the form which is now generally adopted in our treatises.

The explanation of the *Tides*, in the way in which Newton attempted it in the third book of the Principia, is another example of a hydrostatical investigation: for he considered only the form that the ocean would have if it were at rest. The memoirs of Maclaurin, Daniel Bernoulli, and Euler, on the question of the tides, which shared among them the prize of the Academy of Sciences in 1740, went upon the same views.

The Treatise of the Figure of the Earth by Clairaut, in 1743, extended Newton's solution of the same problem, by supposing a solid nucleus covered with a fluid of different density. No peculiar novelty has been introduced into this subject, except a method employed by Laplace for determining the attractions of spheroids of small eccentricity, which is, as Professor Airy has said[11], "a calculus the most singular in its nature, and the most powerful in its effects, of any which has yet appeared."

12. *Capillary Action.*—There is only one other problem of the statics of fluids, on which it is necessary to say a word,—the doctrine of capillary attraction. Daniel Bernoulli[12], in 1738, states that he passes over the subject, because he could not reduce

[11] Enc. Met. Fig. of Earth, p. 192.

[12] Hydrodyn. pref. p. 5.

the facts to general laws: but Clairaut was more successful, and Laplace and Poisson have since given great analytical completeness to his theory. At present our business is, not so much with the sufficiency of the theory to explain phenomena, as with the mechanical problem of which this is an example, which is one of a very remarkable and important character; namely, to determine the effect of attractions which are exercised by all the particles of bodies, on the hypothesis that the attraction of each particle, though sensible when it acts upon another particle at an extremely small distance from it, becomes insensible and vanishes the moment this distance assumes a perceptible magnitude. It may easily be imagined that the analysis by which results are obtained under conditions so general and so peculiar, is curious and abstract: the problem has been resolved in some very extensive cases.

13. *Motion of Fluids.*—The only branch of mathematical mechanics which remains to be considered, is that which is, we may venture to say, hitherto incomparably the most incomplete of all;—Hydrodynamics. It may easily be imagined that the mere hypothesis of absolute relative mobility in the parts, combined with the laws of motion and nothing more, are conditions too vague and general to lead to definite conclusions. Yet such are the conditions of the problems which relate to the motion of fluids. Accordingly, the mode of solving them has been, to introduce certain other hypotheses, often acknow-

ledged to be false, and almost always in some measure arbitrary, to assist in determining and obtaining the solution. The velocity of a fluid issuing from an orifice in a vessel, and the resistance which a solid body suffers in moving in a fluid, have been the two main problems on which mathematicians have employed themselves. We have already spoken of the manner in which Newton attacked both these, and endeavoured to connect them. The subject became a branch of analytical mechanics by the labours of D. Bernoulli, whose *Hydrodynamica* was published in 1738. This work rests upon the Huyghenian principle of which we have already spoken in the history of the centre of oscillation; namely, the equality of the actual descent of the particles and the potential ascent; or, in other words, the conservation of *vis viva*. This was the first analytical treatise; and the analysis is declared by Lagrange to be as elegant in its steps as it is simple in its results. Maclaurin also treated the subject; but is accused of reasoning in such a way as to show that he had determined upon his result beforehand; and the method of John Bernoulli, who likewise wrote upon it, has been strongly objected to by D'Alembert. D'Alembert himself applied the principle which bears his name, to this subject; publishing a "Treatise on the Equilibrium and Motion of Fluids" in 1744, and on the "Resistance of Fluids" in 1753. His *Réflexions sur la Cause Générale des Vents*, printed in 1747, are also a cele-

brated work, belonging to this part of mathematics. Euler, in this as in other cases, was one of those who most contributed to give analytical elegance to the subject. In addition to the questions which have been mentioned, he and Lagrange treated the problems of the small vibrations of fluids, both inelastic and elastic;—a subject which leads, like the question of vibrating strings, to some subtle and abstruse considerations concerning the significations of the integrals of partial differential equations. Laplace also took up the subject of waves propagated along the surface of water; and deduced a very celebrated theory of the tides, in which he considered the ocean to be, not in equilibrium, as preceding writers had supposed, but agitated by a constant series of undulations, produced by the solar and lunar forces. The difficulty of such an investigation may be judged of from this, that Laplace, in order to carry it on, is obliged to assume a mechanical proposition, unproved, and only conjectured to be true; namely[13], that "in a system of bodies acted upon by forces which are periodical, the state of the system is periodical like the forces." Even with this assumption, various other arbitrary processes are requisite; and it appears still very doubtful whether Laplace's theory is either a better mechanical solution of the problem, or a nearer approximation to the laws of the phenomena, than that obtained by D. Bernoulli, following the views of Newton.

[13] Méc. Cél. t. ii. p. 218.

In most cases, the solutions of problems of hydrodynamics are not satisfactorily confirmed by the results of observation. Poisson and Cauchy have prosecuted the subject of waves, and have deduced very curious conclusions by a very recondite and profound analysis. The assumptions of the mathematician here do not represent the conditions of nature; the rules of theory, therefore, are not a good standard to which we may refer the aberrations of particular cases; and the laws which we obtain from experiment are very imperfectly illustrated by *à priori* calculation. The case of this department of knowledge, hydrodynamics, is very peculiar; we have reached the highest point of the science,—the laws of extreme simplicity and generality from which the phenomena flow; we cannot doubt that the ultimate principles which we have obtained are the true ones, and those which really apply to the facts; and yet we are far from being able to apply the principles to explain or find out the facts. In order to do this, we want, in addition to what we have, true and useful principles, intermediate between the highest and the lowest;—between the extreme and almost barren generality of the laws of motion, and the endless varieties and inextricable complexity of fluid motions in special cases. The reason of this peculiarity in the science of hydrodynamics appears to be, that its general principles were not discovered with reference to the science itself, but by extension from the sister science of the mechanics of solids: they were not

obtained by ascending gradually from particulars to truths more and more general, respecting the motions of fluids; but were caught at once, by a perception that the parts of fluids are included in that range of generality which we are entitled to give to the supreme laws of motion of solids. Thus, solid and fluid dynamics resemble two edifices which have their highest apartment in common, and though we can explore every part of the former building, we have not yet succeeded in traversing the staircase of the latter, either from the top or from the bottom. If we had lived in a world in which there were no solid bodies, we should probably not yet have discovered the laws of motion; if we had lived in a world in which there were no fluids, we should have no idea how insufficient a complete possession of the laws of motion may be, to give us a true knowledge of particular results.

14. *Various General Mechanical Principles.*—The generalised laws of motion, the points to which I have endeavoured to conduct my history, include in them all other laws by which the motions of bodies can be regulated; and among such, several laws which had been discovered before the highest point of generalisation was reached, and which thus served as stepping-stones to the ultimate principles. Such were, as we have seen, the principles of the conservation of *vis viva* and of the motion of the centre of gravity, and the like. These principles may, of course, be deduced from our elementary laws, and were finally

established by mathematicians on that footing. There are other principles which may be similarly demonstrated; among the rest, I may mention the principle of *the conservation of areas*, which extends to any number of bodies a law analogous to that which Kepler had observed respecting the areas described by each planet round the sun. I may mention also, the principle of the *immobility of the plane of maximum areas*, a plane which is not disturbed by any mutual action of the parts of any system. The former of these principles was published about the same time by Euler, D. Bernoulli, and Darcy, under different forms, in 1746 and 1747; the latter by Laplace.

To these may be added a law, very celebrated in its time, and the occasion of an angry controversy, *the principle of least action*. Maupertuis conceived that he could establish *à priori*, by theological arguments, that all mechanical changes must take place in the world so as to occasion the least possible quantity of *action*. In asserting this, it was proposed to measure the action by the product of velocity and space; and this measure being adopted, the mathematicians, though they did not generally assent to Maupertuis' reasonings, found that his principle expressed a remarkable and useful truth, which might be established on known mechanical grounds.

15. *Analytical Generality. Connexion of Statics and Dynamics.*—Before I quit this subject, it is important to remark the peculiar character which the science of mechanics has now assumed, in con-

sequence of the extreme analytical generality which has been given it. Symbols, and operations upon symbols, include the whole of the reasoner's task; and though the relations of space are the leading subjects in the science, the great analytical treatises upon it do not contain a single diagram. The "Mécanique Analytique" of Lagrange, of which the first edition appeared in 1788, is by far the most consummate example of this analytical generality. "The plan of this work," says the author, "is entirely new. I have proposed to myself to reduce the whole theory of this science, and the art of resolving the problems which it includes, to general formulæ, of which the simple developement gives all the equations necessary for the solution of the problem."—"The reader will find no figures in the work. The methods which I deliver do not require either constructions, or geometrical or mechanical reasonings; but only algebraical operations, subject to a regular and uniform rule of proceeding." Thus this writer makes mechanics a branch of analysis; instead of making, as had previously been done, analysis an implement of mechanics. The transcendent generalising genius of Lagrange, and his matchless analytical skill and elegance, have made this undertaking as successful as it is striking.

The mathematical reader is aware that the language of mathematical symbols is, in its nature, more general than the language of words; and that in this way truths, translated into symbols, often

suggest their own generalisations. Something of this kind has happened in mechanics. The same formula expresses the general condition of statics and that of dynamics. The tendency to generalisation which is thus introduced by analysis, makes mathematicians unwilling to acknowledge a plurality of mechanical principles; and in the most recent analytical treatises on the subject, all the doctrines are deduced from the single law of inertia. Indeed, if we identify forces with the velocities which produce them, and allow the composition of forces to be applicable to force *so understood*, it is easy to see that we can reduce the laws of motion to the principles of statics; and this conjunction, though it may not be considered as philosophically just, is verbally correct. If we thus multiply or extend the meanings of the term force, we make our elementary principles simpler and fewer than before; and those persons, therefore, who are willing to assent to such a use of words, can thus obtain an additional generalisation of dynamical principles; and this, as I have stated, has been adopted in several recent treatises. I shall not further discuss here how far this is a real advance in science.

Having thus rapidly gone through the history of force and attraction in the abstract, we return to the attempt to interpret the phenomena of the universe by the aid of these abstractions thus established.

NOTE ON LEONARDO DA VINCI.

When the preceding pages were prepared for the press, I had not seen Venturi's "Essai sur les Ouvrages Physico-Mathématiques de Léonard da Vinci, avec des Fragmens tirés de ses Manuscrits apportés d'Italie. Paris, 1797." Leonardo was born in 1452, and died in 1519; and was an eminent mathematician and engineer, as well as a painter, sculptor, and architect. It is proper to examine, therefore, whether he claims a place in the history of astronomy and mechanics. The following statements will show that he is no inconsiderable figure in the prelude to the great discoveries in both these sciences; if, indeed, we do not put him in the stead of Stevinus, as the first person who clearly understood the oblique action of pressure.

Leonardo da Vinci, about 1510, explained how a body, by describing a kind of spiral, might descend towards a revolving globe, so that its apparent motion, relatively to a point in the surface, might be in a straight line tending to the centre. He thus showed that he had entertained in his thoughts the hypothesis of the earth's rotation, and was employed in removing the difficulties which accompanied it, by the consideration of the composition of motions.

He also, as early as 1499, gave a perfectly correct statement of the proportion of the forces exerted by a cord which acts obliquely and supports a weight on a lever. He distinguishes between the real lever, and the *potential levers*, that is, the perpendiculars drawn from the centre upon the directions of the forces. Nothing can be more entirely sound and satisfactory than this. It is quite as good as the proof of Stevinus. These views must, in all probability, have been sufficiently promulgated by the time of Galileo, to influence his reasonings concerning the lever; which, indeed, much resemble those of Leonardo.

Da Vinci also anticipated Galileo in *asserting* that the time of descent of a body down an inclined plane is to the time of descent down its vertical height, in the proportion of the length

of the plane to its height. But this cannot, I think, have been more than a guess: there is no vestige of a proof given.

The general reflection which these quotations suggest, is that both the heliocentric doctrine and truths of mechanics were fermenting in the minds of intelligent men, and gradually assuming clearness and strength, some time before they were publicly asserted.

BOOK VII.

THE MECHANICAL SCIENCES.

(CONTINUED.)

HISTORY

OF

PHYSICAL ASTRONOMY.



DESCEND from heaven, Urania, by that name
If rightly thou art called, whose voice divine
Following, above the Olympian hill I soar,
Above the flight of Pegasean wing.
The meaning, not the name, I call, for thou
Nor of the muses nine, nor on the top
Of old Olympus dwell'st: but heavenly-born,
Before the hills appeared, or fountain flowed,
Thou with Eternal Wisdom didst converse,
Wisdom, thy sister.

Paradise Lost, B. vii.

CHAPTER I.

Prelude to the Inductive Epoch of Newton.

We have now to contemplate the last and most splendid period of the progress of astronomy;—the grand completion of the history of the most ancient and prosperous province of human knowledge;—the steps which elevated this science to an unrivalled eminence above other sciences;—the first great example of a wide and complex assemblage of phenomena indubitably traced to their single simple cause;—in short, the first example of the formation of a perfect inductive science.

In this, as in other considerable advances in real science, the complete disclosure of the new truths by the principal discoverer, was preceded by movements and glimpses, by trials, seekings, and guesses on the part of others; by indications, in short, that men's minds were already carried by their intellectual impulses in the direction in which the truth lay, and were beginning to detect its nature. In a case so important and interesting as this, it is more peculiarly proper to give some view of this prelude to the epoch of the full discovery.

(*Francis Bacon.*) That astronomy should become physical astronomy,—that the motions of the heavenly bodies should be traced to their causes, as well as re-

duced to rule,—was felt by all persons of active and philosophical minds as a pressing and irresistible need, at the time of which we speak. We have already seen how much this feeling had to do in impelling Kepler to the train of laborious research by which he made his discoveries. Perhaps it may be interesting to point out how strongly this persuasion of the necessity of giving a physical character to astronomy, had taken possession of the mind of Bacon, who, looking at the progress of knowledge with a more comprehensive spirit, and from a higher point of view than Kepler, could have none of his astronomical prejudices, since on that subject he was of a different school, and of far inferior knowledge. In his "Description of the Intellectual Globe," Bacon says that while astronomy had hitherto had it for her business to inquire into the rules of the heavenly motions, and philosophy, into their causes, they had hitherto worked without due appreciation of their respective tasks; philosophy neglecting facts, and astronomy claiming assent to her mathematical hypotheses, which ought to be considered as mere steps of calculation. "Since, therefore," he continues[1], "each science has hitherto been a slight and ill-constructed thing, we must assuredly take a firmer stand; our ground being, that these two subjects, which on account of the narrowness of men's views and the traditions of professors have been so long dissevered,

[1] Vol. ix. 221.

are, in fact, one and the same thing, and compose one body of science." It must be allowed that, however erroneous might be the points of Bacon's positive astronomical creed, these general views of the nature and position of the science are most sound and philosophical.

(*Kepler.*) In his attempts to suggest a right physical view of the starry heavens and their relation to the earth, Bacon failed, along with all the writers of his time. It has already been stated that the main cause of this failure was the want of a knowledge of the true theory of motion;—the non-existence of the science of dynamics. At the time of Bacon and Kepler, it was only just beginning to be possible to trace the heavenly motions to the laws of earthly motion, because the latter were only just then divulged. Accordingly, we have seen that the whole of Kepler's physical speculations proceed upon an ignorance of the first law of motion, and assume it to be the main problem of the physical astronomer to assign the cause which *keeps up* the motions of the planets. Kepler's doctrine is, that a certain force or virtue resides in the sun, by which all bodies within his influence are carried round him. He illustrates[1] the nature of this virtue in various ways, comparing it to light and to the magnetic power, which it resembles in the circumstances of operating at a distance, and also in exercising a

[1] De Stellâ Martis, P. 3, c. xxxiv.

feebler influence as the distance becomes greater. But it was obvious that these comparisons were very imperfect; for they do not explain how the sun produces in a body at a distance a motion *athwart* the line of emanation; and though Kepler introduced an assumed rotation of the sun on his axis as the cause of this effect, that such a cause could produce the result could not be established by any analogy of terrestrial motions. But another image to which he referred, suggested a much more substantial and conceivable kind of mechanical action by which the celestial motions might be produced, namely, a current of fluid matter circulating round the sun, and carrying the planet with it, like a boat in a stream. In the Table of Contents of the work on the planet Mars, the purport of the chapter to which I have alluded is stated as follows: "A physical speculation, in which it is demonstrated that the vehicle of that virtue which urges the planets, circulates through the spaces of the universe after the manner of a river or whirlpool (*vortex*,) moving quicker than the planets." I think it will be found, by any one who reads Kepler's phrases concerning the *moving force*, —*the magnetic nature*,—*the immaterial virtue* of the sun, that they convey no distinct conception, except so far as they are interpreted by the expressions just quoted. A vortex of fluid constantly whirling round the sun, kept in this whirling motion by the rotation of the sun himself, and carrying the planets round the sun by its revolution, as a whirlpool carries

straws, could be readily understood; and though it appears to have been held by Kepler that this current and vortex was immaterial, he ascribes to it the power of overcoming the inertia of bodies, and of putting them and keeping them in motion, the only material properties with which he had anything to do. Kepler's physical reasonings, therefore, amount, as we see, in fact, to the doctrine of vortices round the central bodies, and are occasionally so stated by himself; though by asserting these vortices to be "an immaterial species," and by the fickleness and variety of his phraseology on the subject, he leaves this theory in some confusion;—a proceeding, indeed, which both his want of sound mechanical conceptions, and his busy and inventive fancy, might have led us to expect. Nor, we may venture to say, was it easy for any one at Kepler's time to devise a more plausible theory than the theory of vortices might have been made. It was only with the formation and progress of the science of mechanics that this theory became untenable.

(*Descartes.*) But if Kepler might be excused, or indeed admired, for propounding the theory of vortices at his time, the case was different when the laws of motion had been fully developed, and when those who knew the state of mechanical science ought to have learned to consider the motions of the stars as a mechanical problem, subject to the same conditions as other mechanical problems, and capable of the same exactness of solution. And there was an

especial inconsistency in the circumstance of the theory of vortices being put forwards by Descartes, who pretended, or was asserted by his admirers, to have been one of the discoverers of the true laws of motion. It certainly shows both great conceit and great shallowness, that he should have put forwards with much pomp this crude invention of the ante-mechanical period, at the time when the best mathematicians of Europe, as Borelli in Italy, Hooke and Wallis in England, Huyghens in Holland, were patiently labouring to bring the mechanical problem of the universe into its most distinct form, in order that it might be solved at last and for ever.

I do not mean to assert that Descartes borrowed his doctrines from Kepler, or from any of his predecessors; for the theory was sufficiently obvious; and especially if we suppose the inventor to seek his suggestions rather in the casual examples offered to the sense than in the exact laws of motion. Nor would it be reasonable to rob this philosopher of that credit, of the plausible deduction of a vast system from apparently simple principles, which, at the time, was so much admired; and which undoubtedly was the great cause of the many converts to his views. At the same time we may venture to say that a doctrine thus deduced from assumed principles by a long chain of deduction, not verified and confirmed at every step by detailed and exact facts, has hardly a chance of containing any truth. Descartes said that he should think it little to show how the world

is constructed, if he could not also show that it *must* of necessity have been so constructed. The more modest philosophy which has survived the boastings of his school is content to receive all its knowledge of facts from experience, and never dreams of interposing its peremptory *must be* when nature is ready to tell us what *is*. The *à priori* philosopher has, however, always a strong feeling in his favour among men. The deductive form of his speculations gives them something of the charm and the apparent certainty of pure mathematics; and while he avoids that laborious recurrence to experiments, and measures, and multiplied observations, which is irksome and distasteful to those who are impatient to grow wise at once, every fact of which the theory appears to give an explanation, seems to be an unasked and almost an infallible witness in its favour.

My business with Descartes here is only with his physical theory of vortices; which, great as was its glory at one time, is now utterly extinguished. It was propounded in his *Principia Philosophiæ*, in 1644. In order to arrive at this theory, he begins, as might be expected, from reasonings sufficiently general. He lays it down as a maxim, in the first sentence of his book, that a person who seeks for truth must, once in his life, doubt of all that he most believes. Conceiving himself thus to have stripped himself of all his belief on all subjects, in order to resume that part of it which merits to be retained, he begins with his celebrated assertion, "I

think, therefore I am;" which appears to him a certain and immoveable principle, by means of which he may proceed to something more. Accordingly, to this he soon adds the idea, and hence the certain existence, of God and his perfections. He then asserts it to be also manifest, that a vacuum in any part of the universe is impossible; the whole must be filled with matter, and the matter must be divided into equal angular parts, this being the most simple, and therefore the most natural supposition[3]. This matter being in motion, the parts are necessarily ground into a spherical form; and the corners thus rubbed off (like filings or sawdust) form a second and more subtle matter[4]. There is, besides, a third kind of matter, of parts more coarse and less fitted for motion. The first matter makes luminous bodies, as the sun, and the fixed stars; the second is the transparent substance of the skies; the third is the material of opaque bodies, as the earth, planets, and comets. We may suppose, also[5], that the motions of these parts take the form of revolving circular currents[6], or *vortices*. By this means, the first matter will be collected to the centre of each vortex, while the second, or subtle matter, surrounds it, and, by its centrifugal effort, constitutes light. The planets are carried round the sun by the motion of his vortex[7], each planet being at such a distance from the sun as

[3] Prin. p. 58. [4] Ib. p. 59. [5] Ib. p. 56.
[6] Ib. p. 61. [7] c. 140, p. 114.

to be in a part of the vortex suitable to its solidity and mobility. The motions are prevented from being exactly circular and regular by various causes; for instance, a vortex may be pressed into an oval shape by contiguous vortices. The satellites are, in like manner, carried round their primary planets by subordinate vortices; while the comets have sometimes the liberty of gliding out of one vortex into the one next contiguous, and thus travelling in a sinuous course, from system to system, through the universe.

It is not necessary for us to speak here of the entire deficiency of this system in mechanical consistency, and in a correspondency to observation in details and measures. Its general reception and temporary sway, in some instances even among intelligent men and good mathematicians, are the most remarkable facts connected with it. These may be ascribed, in part, to the circumstance that philosophers were now ready and eager for a physical astronomy commensurate with the existing state of knowledge; they may have been owing also, in some measure, to the character and position of Descartes. He was a man of high claims in every department of speculation, and, in pure mathematics, a genuine inventor of great eminence;—a man of family and a soldier;—an inoffensive philosopher, attacked and persecuted for his opinions with great bigotry and fury, by a Dutch divine, Voet;—the favourite and teacher of two distinguished princesses, and, it is

said, the lover of one of them. This was Elizabeth, the daughter of the Elector Frederick, and consequently grand-daughter of our James the First. His other royal disciple, the celebrated Christina of Sweden, showed her zeal for his instructions by appointing the hour of five in the morning for their interviews. This, in the climate of Sweden, and in the winter, was too severe a trial for the constitution of the philosopher, born in the sunny valley of the Loire; and, after a short residence at Stockholm, he died of an inflammation of the chest in 1650. He always kept up an active correspondence with his friend Mersenne, who was called, by some of the Parisians, "the Resident of Descartes at Paris;" and who informed him of all that was done in the world of science. It is said that he at first sent to Mersenne an account of a system of the universe which he had devised, which went on the assumption of a vacuum; Mersenne informed him that the *vacuum* was no longer the fashion at Paris; upon which he proceeded to remodel his system, and to re-establish it on the principle of a *plenum*. Undoubtedly he tried to avoid promulgating opinions which might bring him into trouble. He, on all occasions, endeavoured to explain away the doctrine of the motion of the earth, so as to evade the scruples to which the decrees of the pope had given rise; and, in stating the theory of vortices, he says[s], "There is

[s] Princ. p. 56.

no doubt that the world was created at first with all its perfection; nevertheless, it is well to consider how it might have arisen from certain principles, although we know that it did not." Indeed, in the whole of his philosophy, he appears to deserve the character of being both rash and cowardly, "*pusillanimus simul et audax*[9]," far more than Aristotle, to whose physical speculations Bacon applies this description.

Whatever the causes might be, his system was well received and rapidly adopted. Gassendi, indeed, says that he found nobody who had the courage to read the Principia through[10]; but the system was soon embraced by the younger professors, who were eager to dispute in its favour. It is said[11] that the University of Paris was on the point of publishing an edict against these new doctrines, and was only prevented from doing so by a pasquinade which is worth mentioning. It was composed by the poet Boileau (about 1684), and professed to be a Request in favour of Aristotle, and an Edict issued from Mount Parnassus in consequence. It is obvious that, at this time, the cause of Cartesianism was looked upon as the cause of free inquiry and modern discovery, in opposition to that of bigotry, prejudice, and ignorance. Probably the poet was far from being a very severe or profound critic of the truth of such claims.

[9] Bacon. vol. ix. p. 230. [10] Del. A. M. ii. 193.
[11] Enc. Brit. Cartesianism.

"This petition of the Masters of Arts, Professors, and Regents of the University of Paris, humbly showeth, that it is of public notoriety that the sublime and incomparable Aristotle was, without contest, the first founder of the four elements, fire, air, earth, and water; that he did, by special grace, accord unto them a simplicity which belongeth not to them of natural right;" and so on. "Nevertheless, since, a certain time past, two individuals, named Reason and Experience, have leagued themselves together to dispute his claim to the rank which of justice pertains to him, and have tried to erect themselves a throne on the ruins of his authority; and, in order the better to gain their ends, have excited certain factious spirits, who, under the names of Cartesians and Gassendists, have begun to shake off the yoke of their master, Aristotle; and, contemning his authority, with unexampled temerity, would dispute the right which he had acquired of making true pass for false and false for true;—" In fact this production does not exhibit any of the peculiar tenets of Descartes, although, probably, the positive points of his doctrines obtained a footing in the University of Paris, under the cover of this assault on his adversaries. The Physics of Rohault, a zealous disciple of Descartes, was published at Paris about 1670[13], and was, for a time, the standard book for students of this subject, both in France and in Eng-

[13] And a second edition in 1672.

land. I do not here speak of the later defenders of the Cartesian system, for, in their hands, it was much modified by the struggle which it had to maintain against the Newtonian system.

We are concerned with Descartes and his school only as they form part of the picture of the intellectual condition of Europe just before the publication of Newton's discoveries. Beyond this, the Cartesian speculations are without value. When, indeed, Descartes' countrymen could no longer refuse their assent and admiration to the Newtonian theory, it came to be the fashion among them to say that Descartes had been the necessary precursor of Newton; and to adopt a favourite saying of Leibnitz, that the Cartesian philosophy was the antechamber of Truth. Yet this comparison is far from being happy: it appeared rather as if these suitors had mistaken the door; for those who first came into the presence of Truth herself, were those who never entered this imagined antechamber, and those who were in the antechamber first, were the last in penetrating further. In partly the same spirit, Playfair has noted it as a service which Newton perhaps owed to Descartes, that "he had exhausted one of the most tempting forms of error." We shall see soon that this temptation had no attraction for those who looked at the problem in its true light, as the Italian and English philosophers already did. Voltaire has observed, far more truly, that Newton's edifice rested on no stone of Descartes' foundations.

He illustrates this by relating that Newton only once read the work of Descartes, and, in doing so, wrote the word "*error*," repeatedly, on the first seven or eight pages; after which he read no more. This volume, Voltaire adds, was, for some time in the possession of Newton's nephew[13].

(*Gassendi.*) Even in his own country, the system of Descartes was by no means universally adopted. We have seen that though Gassendi was coupled with Descartes as one of the leaders of the new philosophy, he was far from admiring his work. Gassendi's own views of the causes of the motions of the heavenly bodies are not very clear, nor even very clearly referrible to the laws of mechanics; although he was one of those who had most share in showing that those laws apply to astronomical motions. In a chapter, headed[14] "Quæ sit motrix siderum causa," he reviews several opinions; but the one which he seems to adopt, is that which ascribes the motion of the celestial globes to certain fibres, of which the action is similar to that of the muscles of animals. It does not appear, therefore, that he had distinctly apprehended, either the continuation of the movements of the planets by the first law of motion, or their deflection by the second law;—the two main steps on the road to the discovery of the true forces by which they are made to describe their orbits.

(*Leibnitz, &c.*) Nor does it appear that in Ger-

[13] Cartesianism, Enc. Phil. [14] Gassendi, Opera, vol. i. p. 639.

many mathematicians had attained this point of view. Leibnitz, as we have seen, did not assent to the opinions of Descartes, as containing the complete truth; and yet his own views of the physics of the universe do not seem to have any great advantage over these. In 1671 he published "A new physical hypothesis, by which the causes of most phenomena are deduced from a certain single universal motion supposed in our globe;—not to be despised either by the Tychonians or the Copernicans." He supposes the particles of the earth to have separate motions, which produce collisions, and thus propagate[15] an "agitation of the ether," radiating in all directions; and[16], "by the rotation of the sun on its axis, concurring with its rectilinear action on the earth, arises the motion of the earth about the sun." The other motions of the solar system are, as we might expect, accounted for in a similar manner; but it appears difficult to invest such a hypothesis with any mechanical consistency.

John Bernoulli maintained to the last the Cartesian hypothesis, though with several modifications of his own, and even pretended to apply mathematical calculation to his principles. This, however, belongs to a later period of our history; to the reception, not to the prelude, of the Newtonian theory.

(*Borelli.*) In Italy, Holland, and England, mathematicians appear to have looked much more steadily

[15] Art. 5. [16] Ib. 8.

at the problem of the celestial motions, by the light which the discovery of the real laws of motion threw upon it. In Borelli's "Theories of the Medicean Planets," printed at Florence in 1666, we have already a conception of the nature of central action, in which true notions begin to appear. The attraction of a body upon another which revolves about it is spoken of, and likened to magnetic action; not converting the attracting force into a transverse force, according to the erroneous views of Kepler, but taking it as a tendency of the bodies to meet. "It is manifest," says he[17], "that every planet and satellite revolves round some principal globe of the universe as a fountain of virtue, which so draws and holds them, that they cannot by any means be separated from it, but are compelled to follow it wherever it goes, in constant and continuous revolutions." And, further on, he describes[18] the nature of the action, as a matter of conjecture indeed, but with remarkable correctness[19]. "We shall account for these motions by supposing, that which can hardly be denied, that the planets have a certain natural appetite for uniting themselves with the globe round which they revolve, and that they really tend, with all their efforts, to approach to such globe; the planets, for instance, to the sun, the Medicean stars to Jupiter. It is certain, also, that circular motion gives a body a tendency to recede from the centre

[17] Cap. 2. [18] Ib. 11. [19] p. 47.

of such revolution, as we find in a wheel, or a stone whirled in a sling. Let us suppose, then, the planet to endeavour to approach the sun; since, in the mean time, it acquires, by the circular motion, a force to recede from the same central body, it comes to pass, that when those two opposite forces are equal, each compensates the other, and the planet cannot go nearer to the sun nor further from him than a certain determinate space, and thus appears balanced and floating about him."

This is a very remarkable passage; but it will be observed, at the same time, that the author has no distinct conception of the manner in which the change of direction of the planet's motion is regulated from one instant to another; still less do his views lead to any mode of calculating the distance from the central body at which the planet would be thus balanced, or the space through which it might approach to the centre and recede from it. There is a great interval from Borelli's guesses, even to Huyghens' theorems; and a much greater to the beginning of Newton's discoveries.

(*England.*) It is peculiarly interesting to us to trace the gradual approach towards these discoveries which took place in the minds of English mathematicians; and this we can do with tolerable distinctness. Gilbert, in his work, *De Magnete*, printed in 1600, has only some vague notions that the magnetic virtue of the earth in some way determines the direction of the earth's axis, the rate of its diurnal rotation,

and that of the revolution of the moon about it[20]. He died in 1603, and, in his posthumous work, already mentioned, (*De Mundo nostro Sublunari Philosophia nova*, 1651,) we have already a more distinct statement of the attraction of one body by another[21]. "The force which emanates from the moon reaches to the earth, and, in like manner, the magnetic virtue of the earth pervades the region of the moon: both correspond and conspire by the joint action of both, according to a proportion and conformity of motions: but the earth has more effect, in consequence of its superior mass; the earth attracts and repels the moon, and the moon, within certain limits, the earth; not so as to make the bodies come together, as magnetic bodies do, but so that they may go on in a continuous course." Though this phraseology is capable of representing a good deal of the truth, it does not appear to have been connected, in the author's mind, with any very definite notions of mechanical action in detail. We may probably say the same of Milton's language:

> What if the sun
> Be centre to the world; and other stars,
> By his attractive virtue and their own
> Incited, dance about him various rounds?
>
> *Par. Lost*, B. viii.

Boyle, about the same period, seems to have inclined to the Cartesian hypothesis. Thus, in order

[20] Lib. vi. cap. 6, 7. [21] Ib. ii. c. 19.

to show the advantage of the natural theology which contemplates organic contrivances, over that which refers to astronomy, he remarks, "it may be said, that in bodies inanimate[**], the contrivance is very rarely so exquisite but that the various motions and occurrences of their parts may, without much improbability, be suspected capable, after many essays, to cast one other into several of those circumvolutions called by Epicurus, συστροφὰς, and by Descartes, *vortices;* which being once made, may continue a long time after the manner explained by the latter." Neither Milton nor Boyle, however, can be supposed to have had an exact knowledge of the laws of mechanics; and therefore they do not fully represent the views of their mathematical contemporaries. But there arose about this time a group of philosophers, who began to knock at the door where Truth was to be found, although it was left for Newton to force it open. These were the founders of the Royal Society, Wilkins, Wallis, Ward, Wren, Hooke, and others. The time of the beginning of the speculations and association of these men corresponds to the time of the civil wars between the king and parliament in England; and it does not appear a fanciful account of their scientific zeal and activity, to say, that while they shared the common mental ferment of the times, they sought in the calm and peaceful pursuit of knowledge a contrast to the vexatious and angry struggles which at that time

[**] Shaw's Boyle's Works, ii. 160.

disturbed the repose of society. It was well if these dissensions produced any good to science to balance the obvious evils which flowed from them. Crabtree, the friend of Horrox, is supposed to have perished in the civil wars; the papers of Horrox himself were burnt, after his death, by a marauding party of soldiers; the anatomical collections of Harvey were plundered and destroyed. Most of these persons of whom we now speak, were involved in the changes of fortune of the Commonwealth, some on one side and some on the other. Wilkins was made warden of Wadham by the committee of parliament appointed for reforming the University of Oxford; and was in 1659 made master of Trinity College, Cambridge, by Richard Cromwell, but ejected thence the year following, upon the restoration of the royal sway. Seth Ward, who was a fellow of Sidney College, Cambridge, was deprived of his fellowship by the parliamentary committee; but at a later period (1649) he took the engagement to be faithful to the Commonwealth, and became Savilian professor of astronomy at Oxford. Wallis held a fellowship of Queen's College, Cambridge, but vacated it by marriage. He was afterwards much employed by the royal party in deciphering secret writings, in which he had peculiar skill. Yet he was appointed by the parliamentary commissioners Savilian professor of geometry at Oxford, in which situation he was continued by Charles II. after his restoration. Wren was some-

what later, and escaped these changes. He was chosen fellow of All-Souls in 1652, and succeeded Ward as Savilian professor of astronomy. These men, along with Boyle and several others, formed themselves into a club, which they called the Philosophical, or the Invisible College; and met, from about the year 1645, sometimes in London, and sometimes in Oxford, according to the changes of fortune and residence of the members. Hooke went to Christ Church, Oxford, in 1653, where he was patronised by Boyle, Ward, and Wallis; and when the Philosophical College resumed its meetings in London after the Restoration, as the Royal Society, Hooke was made "curator of experiments." Halley was of the next generation, and comes after Newton; he studied at Queen's College, Oxford, in 1673; but was at first a man of some fortune, and not engaged in any official situation. His talents and zeal, however, made him an active and effective ally in the promotion of science.

The connexion of the persons of whom we have been speaking has a bearing on our subject, for it led, historically speaking, to the publication of Newton's discoveries in physical astronomy. Rightly to propose a problem is no inconsiderable step to its solution; and it was undoubtedly a great advance towards the true theory of the universe to consider the motion of the planets round the sun as a mechanical question, to be solved by a reference to the laws of motion, and by the use

of mathematics. So far the English philosophers appear to have gone before the time of Newton. Hooke, indeed, when the doctrine of gravitation was published, asserted that he had discovered it previously to Newton; and though this pretension could not be maintained, he certainly had perceived that the thing to be done was, to determine the effect of a central force in producing curvilinear motion; which effect, as we have already seen, he illustrated by experiment as early as 1666. Hooke had also spoken more clearly on this subject in "An Attempt to prove the Motion of the Earth from Observations," published in 1674. In this, he distinctly states that the planets would move in straight lines, if they were not deflected by central forces; and that the central attractive power increases in approaching the centre in certain degrees, dependent on the distance. "Now what these degrees are," he adds, "I have not yet experimentally verified;" but he ventures to promise to any one who succeeds in this undertaking, a discovery of the cause of the heavenly motions. He asserted, in conversation, to Halley and Wren, that he had solved this problem, but his solution was never produced. The proposition that the attractive force of the sun varies inversely as the square of the distance from the centre, had already been divined, if not fully established. If the orbits of the planets were circles, this proportion of the forces might be deduced in the same manner as the propositions concerning circular

motion, which Huyghens published in 1673; yet it does not appear that Huyghens made this application of his principles. Newton, however, had already made this step some years before this time. Accordingly, he says in a letter to Halley, on Hooke's claim to this discovery[23], "When Huygenius put out his *Horologium Oscillatorium*, a copy being presented to me, in my letter of thanks I gave those rules in the end thereof a particular commendation for their usefulness in computing the forces of the moon from the earth, and the earth from the sun." He says, moreover, "I am almost confident by circumstances, that Sir Christopher Wren knew the duplicate proportion when I gave him a visit; and then Mr. Hooke, by his book *Cometa*, will prove the last of us three that knew it." Hooke's *Cometa* was published in 1678. These inferences were all connected with Kepler's law, that the times are in the sesquiplicate ratio of the major axes of the orbits. But Halley had also been led to the duplicate proportion by another train of reasoning, namely, by considering the force of the sun as an emanation, which must become more feeble in proportion to the increased spherical surface over which it is diffused, and therefore in the inverse proportion of the square of the distances[24]. In this view of

[23] Biog. Brit., art. Hooke.

[24] Bullialdus, in 1645, had asserted that the force by which the sun "prehendit et harpagat," takes hold of and grapples the planets, must be as the inverse square of the distance.

the matter, however, the difficulty was to determine what would be the motion of a body acted on by such a force, when the orbit is not circular but oblong. The investigation of this case was a problem which, we can easily conceive, must have appeared of very formidable complexity while it was unsolved, and the first of its kind. Accordingly Halley, as his biographer says, "finding himself unable to make it out in any geometrical way, first applied to Mr. Hooke and Sir Christopher Wren, and meeting with no assistance from either of them, he went to Cambridge in August, (1684,) to Mr. Newton, who supplied him fully with what he had so ardently sought."

A paper of Halley's in the Philosophical Transactions for January, 1686, professedly inserted as a preparation for Newton's work, contains some arguments against the Cartesian hypothesis of gravity, which seem to imply that Cartesian opinions had some footing among English philosophers; and we are told by Whiston, Newton's successor in his professorship at Cambridge, that Cartesianism formed a part of the studies of that place. Indeed, Rohault's "Physics" was used as a class-book at that University long after the time of which we are speaking; but the peculiar Cartesian doctrines which it contained were soon superseded by others.

With regard, then, to this part of the discovery, that the force of the sun follows the inverse duplicate proportion of the distances, we see that several

other persons were on the verge of it at the same time with Newton; though he alone possessed that combination of distinctness of thought and power of mathematical invention, which enabled him to force his way across the barrier. But another, and so far as we know, an earlier train of thought, led by a different path to the same result; and it was the convergence of these two lines of reasoning that brought the conclusion to men's minds with irresistible force. I speak now of the identification of the force which retains the moon in her orbit with the force of gravity by which bodies fall at the earth's surface. In this comparison Newton had, so far as I am aware, no forerunner. We are now, therefore, arrived at the point at which the history of Newton's great discovery properly begins.

CHAPTER II.

The Inductive Epoch of Newton.—Discovery of the Universal Gravitation of Matter, according to the Law of the Inverse Square of the Distance.

In order that we may the more clearly consider the bearing of this, the greatest scientific discovery ever made, we shall resolve it into the partial propositions of which it consists. Of these we may enumerate five. The doctrine of universal gravitation asserts,

1. That the force by which the *different* planets are attracted to the sun is in the inverse proportion of the squares of their distances;

2. That the force by which the *same* planet is attracted to the sun, in different parts of its orbit, is also in the inverse proportion of the inverse squares of the distances;

3. That the *earth* also exerts such a force on the *moon*, and that this force is identical with the force of *gravity;*

4. That bodies thus act on *other* bodies, besides those which revolve round them; thus, that the sun exerts such a force on the moon and satellites, and that the planets exert such forces on *one another;*

5. That this force, thus exerted by the general

masses of the sun, earth, and planets, arises from the attraction *of each particle* of these masses; which attraction follows the above law, and belongs to all matter alike.

The history of the establishment of these five truths will be given in order.

1. *Sun's Force on Different Planets.*—With regard to the first of the above five propositions, that the different planets are attracted to the sun by a force which is inversely as the square of the distance, Newton had so far been anticipated, that several persons had discovered it to be true, or nearly true; that is, they had discovered that if the orbits of the planets were circles, the proportions of the central force to the inverse square of the distance would follow from Kepler's third law, of the sesquiplicate proportion of the periodic times. As we have seen, Huyghens' theorems would have proved this, if they had been so applied; Wren knew it; Hooke not only knew it, but claimed a prior knowledge to Newton; and Halley had satisfied himself that it was at least nearly true, before he visited Newton. Hooke was reported to Newton at Cambridge, as having applied to the Royal Society to do him justice with regard to his claims; but when Halley wrote and informed Newton (letter dated June 29, 1686), that Hooke's conduct "had been represented in worse colours than it ought," Newton inserted in his book a notice of these his predecessors[1], in order,

[1] Biog. Brit. folio, art. Hooke.

as he said, "to compose the dispute." This notice appears in a Scholium to the fourth Proposition of the Principia, which states the general law of revolutions in circles. "The case of the sixth corollary," Newton there says, "obtains in the celestial bodies, as has been separately inferred by our countrymen, Wren, Hooke, and Halley;" he soon after names Huyghens, "who, in his excellent treatise 'De Horologio Oscillatorio,' compares the force of gravity with the centrifugal forces of revolving bodies."

The two steps requisite for this discovery were, to propose the motions of the planets as simply a mechanical problem, and to apply mathematical reasoning so as to solve this problem, with reference to Kepler's third law considered as a fact. The former step was the result of the mechanical discoveries of Galileo and his school, of the firm and clear place which these gradually obtained in men's minds, and of the utter abolition of all the notions of solid spheres by Kepler. The mathematical step required no small mathematical powers; as appears, when we consider that this was the first example of such a problem, and that the method of limits, under all its forms, was at this time in its infancy, or rather, at its birth. Accordingly, even this step, though much the easiest in the path of deduction, no one till Newton completely executed.

2. *Force in different Points of an Orbit.*—The inference of the law of the force from Kepler's two laws concerning the elliptical motion, was a problem

quite different from the preceding, and much more difficult; but the dispute with respect to priority in the two propositions was intermingled. Borelli in 1666, had, as we have seen, endeavoured to reconcile the general form of the orbit with the notion of a central attractive force, by taking centrifugal force into the account; and Hooke, in 1679, had asserted that the result of the law of the inverse square in the force of the earth would be an ellipse[2], or a curve like an ellipse[3]. But it does not appear that this was anything more than a conjecture. Halley says[4] that "Hooke, in 1683, told him he had demonstrated all the laws of the celestial motions by the reciprocally duplicate proportion of the force of gravity; but that, being offered forty shillings by Sir Christopher Wren to produce such a demonstration, his answer was, that he had it, but would conceal it for some time, that others, trying and failing, might know how to value it when he should make it public." Halley, however, truly observes, that after the publication of the demonstration in the Principia, this reason no longer held; and adds, "I have plainly told him, that unless he produce another differing demonstration, and let the world judge of it, neither I nor any one else can believe it."

Newton allows that Hooke's assertions in 1679 gave occasion to his investigations on this point of

[2] Newton's Letter, Biog. Brit., Hooke, p. 2660.
[3] Birch's Hist. R. S., Wallis's Life.
[4] Enc. Brit., Hooke, p. 2660.

the theory. His demonstration is contained in the second and third Sections of the Principia. He first treats of the general law of central forces in any curve; and then, on account, as he states, of the application to the motion of the heavenly bodies, he treats of the case of force varying inversely as the square of the distance, in a more diffuse manner.

In this, as in the former portion of his discovery, the two steps were, the proposing the heavenly motions as a mechanical problem, and the solving this problem. Borelli and Hooke had certainly made the former step, with considerable distinctness; but the mathematical solution required no common inventive power.

Newton seems to have been much ruffled by Hooke's speaking slightly of the value of this second step; and is moved in return to deny Hooke's pretensions with some asperity, and to assert his own. He says, in a letter to Halley, "Borelli did something in it, and wrote modestly; he (Hooke) has done nothing; and yet written in such a way as if he knew and had sufficiently hinted all but what remained to be determined by the drudgery of calculations and observations; excusing himself from that labour by reason of his other business; whereas he should rather have excused himself by reason of his inability: for it is very plain, by his words, he knew not how to go about it. Now is not this very fine? Mathematicians that find out, settle, and do all the business, must content themselves with being

nothing but dry calculators and drudges; and another that does nothing but pretend and grasp at all things, must carry away all the inventions, as well of those that were to follow him as of those that went before." This was written, however, under the influence of some degree of mistake; and in a subsequent letter, Newton says, "Now I understand he was in some respects misrepresented to me, I wish I had spared the postscript to my last," in which is the passage just quoted. We see, by the melting away of rival claims, the undivided honour which belongs to Newton, as the real discoverer of the proposition now under notice. We may add, that in the sequel of the third Section of the Principia, he has traced its consequences, and solved various problems flowing from it with his usual fertility and beauty of mathematical resource; and has there shown the necessary connexion of Kepler's third law with his first and second.

3. *Moon's Gravity to the Earth.*—Though others had considered cosmical forces as governed by the general laws of motion, it does not appear that they had identified such forces with the force of terrestrial gravity. This step in Newton's discoveries has generally been the most spoken of by superficial thinkers; and a false kind of interest has been attached to it, from the story of its being suggested by the fall of an apple. The popular mind is caught by the character of an eventful narrative which the anecdote gives to this occurrence; and by the anti-

thesis which makes a profound theory appear the result of a trivial accident. How inappropriate is such a view of the matter we shall soon see. The narrative of the progress of Newton's thoughts, is given by Pemberton (who had it from Newton himself) in his preface to his View of Newton's Philosophy, and by Voltaire, who had it from Miss Conduit, Newton's niece[5]. "The first thoughts," we are told, "which gave rise to his Principia, he had when he retired from Cambridge, in 1666, on account of the plague, (he was then twenty-four years of age.) As he sat alone in a garden, he fell into a speculation on the power of gravity; that as this power is not found sensibly diminished at the remotest distance from the centre of the earth to which we can rise, neither at the tops of the loftiest buildings, nor even on the summits of the highest mountains, it appeared to him reasonable to conclude that this power must extend much further than was usually thought: Why not as high as the moon? said he to himself; and if so, her motion must be influenced by it; perhaps she is retained in her orbit thereby."

The thought of cosmical gravitation was thus distinctly brought into being: and Newton's superiority here was, that he conceived the celestial motions as distinctly as the motions which took place close to him;—considered them as of the

[5] Elemens de Phil. de Newton. 3me partie, chap. iii.

same kind, and applied the same rules to each, without hesitation or obscurity. But so far, this thought was merely a guess: its occurrence showed the activity of the thinker; but to give it any value, it required much more than a "why not?"—a "perhaps." Accordingly, Newton's "why not?" was immediately succeeded by his "if so, what then?" His reasoning was, that if gravity reach to the moon, it is probably of the same kind as the central force of the sun, and follows the same rule with respect to the distance. What is this rule? We have already seen that, by calculating from Kepler's laws, and supposing the orbits to be circles, the rule of the force appears to be the inverse duplicate proportion of the distance; and this, which had been current as a conjecture among the previous generation of mathematicians, Newton had already proved by indisputable reasonings, and was thus prepared to proceed in his train of inquiry. If then, he went on, pursuing his train of thought, the earth's gravity extend to the moon, diminishing according to the inverse square of the distance, will it, at the moon's orbit, be of the proper magnitude for retaining her in her path? Here again came in calculation, and a calculation of extreme interest; for how important and how critical was the decision which depended on the resulting numbers! According to Newton's calculations, made at this time, the moon, by her motion in her orbit, was deflected from the tangent every minute through a space of thirteen feet. But

by noticing the space which bodies would fall in one minute at the earth's surface, and supposing this to be diminished in the ratio of the inverse square, it appeared that gravity would, at the moon's orbit, draw a body through more than fifteen feet. The difference seems small, the approximation encouraging, the theory plausible; a man in love with his own fancies would readily have discovered or invented some probable cause of this difference. But Newton acquiesced in it as a disproof of his conjecture, and "laid aside at that time any further thoughts of this matter;" thus resigning a favourite hypothesis, with a candour and openness to conviction not inferior to Kepler, though his notion had been taken up on far stronger and sounder grounds than Kepler dealt in; and without even, so far as we know, Kepler's regrets and struggles. Nor was this levity or indifference; the idea, though thus laid aside, was not finally condemned and abandoned. When Hooke, in 1679, contradicted Newton on the subject of the curve described by a falling body, and asserted it to be an ellipse, Newton was led to investigate the subject, and was then again conducted, by another road, to the same law of the inverse square of the distance. This naturally turned his thoughts to his former speculations. Was there really no way of explaining the discrepancy which this law gave, when he attempted to reduce the moon's motion to the action of gravity? A scientific work then recently completed, gave the explanation at once. He had been mis-

taken in the magnitude of the earth, and consequently in the distance of the moon, which is determined by measurements of which the earth's radius is the base. He had taken the common estimate, current among geographers and seamen, that sixty English miles are contained in one degree of latitude. But Picart, in 1670, had measured the length of a certain portion of the meridian in France, with far greater accuracy than had yet been attained; and this measure, then recently published, was mentioned at the meeting of the Royal Society in June, 1682, when Newton was present. Newton took a memorandum of the result obtained by the French astronomer: we may easily imagine the strong curiosity which he must feel to repeat his calculations with these amended data. According to the account which is given by Robison[6], "He went home, took out his old papers, and resumed his calculations. As they drew to a close, he was so much agitated that he was obliged to desire a friend to finish them." His former conjecture was now found to agree with the phenomena to a remarkable degree of precision. This conclusion, thus coming after long doubts and delays, and falling in with the other results of mechanical calculation for the solar system, gave a stamp from that moment to his opinions, and through him to those of the whole philosophical world.

[6] Robison, Phys. Astr. Art. 197. I do not know the authority for this anecdote.

It does not appear, I think, that before Newton philosophers in general had supposed that terrestrial gravity was the very force by which the moon's motions are produced. Men had, as we have seen, taken up the conception of such forces, and had probably called them gravity: but this was done only to explain, by analogy, what *kind* of forces they were, just as at other times they compared them with magnetism; and it did not imply that terrestrial gravity was a force which acted in the celestial spaces. After Newton had discovered that this was so, the application of the term "gravity" did undoubtedly convey such a suggestion; but we should err if we inferred from this coincidence of expression that the notion was commonly entertained before him. Thus Huyghens appears to use language which may be mistaken, when he says[7], that Borelli was of opinion that the primary planets were urged by "gravity" towards the sun, and the satellites towards the primaries. The notion of terrestrial gravity, as being actually a cosmical force, is foreign to all Borelli's speculations[8]. But Horrox, as early as 1635, appears to have entertained the true view on this subject, although vitiated by Keplerian errors concerning the connexion between the rotation of the central body and its effect on the body which revolves about it. Thus he says[9], that the emanation of the earth

[7] Cosmotheoros l. 2. p. 720.

[8] I have found no instance in which the word is so used by him.

[9] Astronomia Kepleriana defensa et promota, cap. 2.

carries a projected stone along with the motion of the earth, just in the same way as it carries the moon in her orbit; and that this force is greater on the stone than on the moon, because the distance is less.

The Proposition in which Newton has stated the discovery of which we are now speaking, is the fourth of his third Book: "That the moon gravitates to the earth, and by the force of gravity is perpetually deflected from a rectilinear motion, and retained in her orbit." The proof consists in the numerical calculation, of which he only gives the elements, and points out the method; but we may observe, that no small degree of knowledge of the way in which astronomers had obtained these elements, and judgment in selecting among them, were necessary: thus, the mean distance of the moon had been made as little as fifty-six and a half semi-diameters of the earth by Tycho, and as much as sixty-two and a half by Kircher: Newton gives good reasons for adopting sixty-one.

The term "gravity," and the expression, "to gravitate," which, as we have just seen, Newton uses of the moon, were to receive a still wider application in consequence of his discoveries; but in order to make this extension clearer, we consider it as a separate step.

4. *Mutual Attraction of all the Celestial Bodies.*— If the preceding parts of the discovery of gravitation were comparatively easy to conjecture, and difficult to prove; this was much more the case with the

part of which we have now to speak, the attraction of other bodies, besides the central ones, upon the planets and satellites. If the mathematical calculation of the unmixed effect of a central force required transcendent talents, how much must the difficulty be increased, when other influences prevented those first results from being accurately verified, while the deviations from accuracy were far more complex than the original action! If it had not been that these deviations, though surprisingly numerous and complicated in their nature, were very small in their quantity, it would have been impossible for the intellect of man to deal with the subject; as it was, the struggle with its difficulties is even now a matter of wonder.

The conjecture that there is some mutual action of the planets, had been put forth by Hooke in his "Attempt to prove the Motion of the Earth," (1674.) It followed, he said, from his doctrine, that not only the sun and the moon act upon the course and motion of the earth, but that Mercury, Venus, Mars, Jupiter, and Saturn, have also, by their attractive power, a considerable influence upon the motion of the earth, and the earth in like manner powerfully affects the motions of those bodies. And Borelli, in attempting to prove "theories" of the satellites of Jupiter, had seen, though dimly and confusedly, the probability that the sun would disturb the motions of these bodies. Thus he says, (cap. 14,) "How can we believe that the Medicean

globes are not, like other planets, impelled with a greater velocity when they approach the sun: and thus they are acted upon by two moving forces, one of which produces their proper revolution about Jupiter, the other regulates their motion round the sun." And in another place, (cap. 20,) he attempts to show an effect of this principle upon the inclination of the orbit; though, as might be expected, without any real result.

The case which most obviously suggests the notion that the sun exerts a power to disturb the motions of secondary planets about primary ones, might seem to be our own moon; for the great inequalities which had hitherto been discovered, had all, except the first, or elliptical anomaly, a reference to the position of the sun. Nevertheless, I do not know that any one had attempted thus to explain the curiously irregular course of the earth's attendant. To calculate, from the disturbing agency, the amount of the irregularities, was a problem which could not, at any former period, have been dreamt of as likely to be at any time within the verge of human power.

Newton both made the step of inferring that there were such forces, and, to a very great extent, calculated the effects of them. The inference is made on mechanical principles, in the sixth Theorem of the third Book of the Principia;—that the moon is attracted by the sun, as the earth is;—that the satellites of Jupiter and Saturn are attracted as the primaries are; in the same manner, and with the

same forces. If this were not so, it is shown that these attendant bodies could not accompany the principal ones in the regular manner in which they do. All those bodies at equal distances from the sun would be equally attracted.

But the complexity which must occur in tracing the results of this principle will easily be seen. The satellite and the primary, though nearly at the same distance, and in the same direction, from the sun, are not exactly so. Moreover the difference of the distances and of the directions is perpetually changing; and if the motion of the satellite be elliptical, the cycle of change is long and intricate: on this account alone the effects of the sun's action will inevitably follow cycles as long and as perplexed as those of the positions. But on another account they will be still more complicated; for in the continued action of a force, the effect which takes place at first, modifies and alters the effect afterwards. The result at any moment is the sum of the results in preceding instants: and since the terms, in this series of instantaneous effects, follow very complex rules, the sums of such series will be; it might be expected, utterly incapable of being reduced to any manageable degree of simplicity.

It certainly does not appear that any one but Newton could make any impression on this problem, or course of problems. No one for sixty years after the publication of the Principia, and, with Newton's methods, no one up to the present day, has added

anything of any value to his deductions. We know that he calculated all the principal lunar inequalities; in many of the cases, he has given us his processes; in others, only his results. But who has presented, in his beautiful geometry, or deduced from his simple principles, any of the inequalities which he left untouched? The ponderous instrument of synthesis, so effective in his hands, has never since been grasped by one who could use it for such purposes; and we gaze at it with admiring curiosity, as on some gigantic implement of war, which stands idle among the memorials of ancient days, and makes us wonder what manner of man he was who could wield as a weapon what we can hardly lift as a burden.

It is not necessary to point out in detail the sagacity and skill which mark this part of the Principia. The mode in which the author obtains the effect of a disturbing force in producing a motion of the apse of an elliptical orbit (the ninth Section of the first Book), has always been admired for its ingenuity and elegance. The general statement of the nature of the principal inequalities produced by the sun in the motion of a satellite, given in the sixty-sixth Proposition, is, even yet, one of the best explanations of such action; and the calculations of the quantity of the effects in the third Book, for instance, the *variation* of the moon, the *motion of the nodes* and its inequalities, the *change of inclination* of the orbit,—are full of beautiful and efficacious artifices. But Newton's inventive faculty was exercised to an extent greater

than these published investigations show. In several cases he has suppressed the demonstration of his method, and given us the result only; either from haste, or from mere weariness, which might well overtake one who, while he was struggling with facts and numbers, with difficulties of conception and practice, was aiming also at that geometrical elegance of exposition, which he considered as alone fit for the public eye. Thus, in stating the effect of the eccentricity of the moon's orbit upon the motion of the apogee, he says[10], "The computations, as too intricate and embarrassed with approximations, I do not choose to introduce."

The computations of the theoretical motion of the moon being thus difficult, and its irregularities numerous and complex, we may ask, whether Newton's reasoning was sufficient to establish this part of his theory; namely, that her actual motions arise from her gravitation to the sun. And to this we may reply, that it was sufficient for that purpose,—since it showed that, from Newton's hypothesis, inequalities must result, following the laws which the moon's inequalities were known to follow;—since the amount of the inequalities given by the theory agreed nearly with the rules which astronomers had collected from observation;—and since, by the very intricacy of the calculation, it was rendered probable, that the first results might be somewhat inaccurate, and thus might give rise to the still remaining differences between the

[10] Schol. to Prop. 35, first edit.

calculations and the facts. A *progression of the apogee;* a *regression of the nodes;* and, besides the elliptical, or first inequality, an inequality, following the law of the *evection*, or second inequality discovered by Ptolemy; another, following the law of the *variation* discovered by Tycho;—were pointed out in the first edition of the Principia, as the consequences of the theory. Moreover, the quantities of these inequalities were calculated and compared with observation with the utmost confidence, and the agreement in most instances was striking. The variation agreed with Halley's recent observations within a minute of a degree[11]. The mean motion of the nodes in a year agreed within less than one-hundredth of the whole[12]. The equation of the motion of the nodes also agreed well[13]. The inclination of the plane of the orbit to the ecliptic, and its changes, according to the different situations of the nodes, likewise agreed[14]. The evection has been already noticed as encumbered with peculiar difficulties; here the accordance was less close. The difference of the daily progress of the apogee in syzygy, and its daily regress in quadratures, is, Newton says, "$4\frac{1}{4}$ minutes by the tables, $6\frac{2}{3}$ by our calculation." He boldly adds, "I suspect this difference to be due to the fault of the tables." In the second edition (1711) he added the calculation of several other inequalities, as the *annual equation*, also discovered by Tycho; and he compared them with more recent observations, made by Flam-

[11] B. iii. Prop. 29. [12] Prop. 32.
[13] Prop. 33. [14] Prop. 35.

steed at Greenwich; but even in what has already been stated, it must be allowed that there is a wonderful accordance of theory with phenomena, both being very complex in the rules which they educe.

The same theory which gave these inequalities in the motion of the moon produced by the disturbing force of the sun, gave also corresponding inequalities in the motions of the satellites of other planets, arising from the same cause; and likewise pointed out the necessary existence of irregularities in the motions of the planets arising from their mutual attraction. Newton gave propositions by which the irregularities of the motion of Jupiter's moons might be deduced from those of our own[15]; and it was shown that the motions of their nodes would be slow by theory, as Flamsteed had found it to be by observation[16]. But Newton did not attempt to calculate the effect of the mutual action of the planets, though he observes, that in the case of Jupiter and Saturn this effect is too considerable to be neglected[17]; and he notices, in the second edition[18], that it follows from the theory of gravity, that the aphelia of Mercury, Venus, the Earth, and Mars, slightly progress.

In one celebrated instance, indeed, the deviation of the theory of the Principia from observation, was wider, and more difficult to explain; and as this deviation for a time resisted the analysis of Euler and

[15] B. i. Prop. 66.
[16] B. iii. Prop. 23.
[17] B. iii. Prop. 13.
[18] Scholium to Prop. 14. B. iii.

Clairaut, as it had resisted the synthesis of Newton, it at one period staggered the faith of mathematicians in the exactness of the law of the inverse square of the distance. I speak of the motion of the moon's apogee, a problem which has already been referred to; and in which Newton's method, and all the methods which could be devised for some time afterwards, gave only half the observed motion; a circumstance which arose, as was discovered by Clairaut in 1750, from the insufficiency of the method of approximation. Newton does not attempt to conceal this discrepancy. After calculating what the motion of apse would be, upon the assumption of a disturbing force of the same amount as that which the sun exerts on the moon, he simply says[19], "the apse of the moon moves about twice as fast."

The difficulty of doing what Newton did in this branch of the subject, and the powers it must have required, may be judged of from what has already been stated;—that no one, with his methods, has yet been able to add anything to his labours: few have undertaken to illustrate what he has written, and no great number have understood it throughout. The extreme complication of the forces, and of the conditions under which they act, makes the subject by far the most thorny walk of mathematics. It is necessary to resolve the action into many elements, such as can be separated; to invent artifices for

[19] B. i. Prop. 44, second edit. There is reason to believe, however, that Newton had, in his unpublished calculations, rectified this discrepancy.

dealing with each of these; and then to recompound the laws thus obtained into one common conception. The moon's motion cannot be conceived without comprehending a scheme more complex than the Ptolemaic epicycles and eccentrics in their worst form; and the component parts of the system are not, in this instance, mere geometrical ideas, requiring only a distinct apprehension of relations of space in order to hold them securely; they are the foundations of mechanical notions, and require to be grasped so that we can apply to them sound mechanical reasonings. Newton's successors, in the next generation, abandoned the hope of imitating him in this intense mental effort; they gave the subject over to the operation of algebraical reasoning, in which symbols think for us, without our dwelling constantly upon their meaning, and obtain for us the consequences which result from the relations of space and the laws of force, however complicated be the conditions under which they are combined. Even Newton's countrymen, though they were long before they applied themselves to the method thus opposed to his, did not produce anything which showed that they had mastered, or could retrace, the Newtonian investigations.

Thus the Problem of Three Bodies, treated geometrically, belongs exclusively to Newton; and the proofs of the mutual action of the sun, planets, and satellites, which depend upon such reasoning, could not be discovered by any one but him.

But we have not yet done with his achievements on this subject; for some of the most remarkable and beautiful of the reasonings which he connected with this problem, belong to the next step of his generalisation.

5. *Mutual Attraction of all Particles of Matter.*—That all the parts of the universe are drawn and held together by love, or harmony, or some affection to which, among other names, that of *attraction* may have been given, is an assertion which may very possibly have been made at various times, by speculators writing at random, and taking their chance of meaning and truth. The authors of such casual dogmas have generally nothing accurate or substantial, either in their conception of the general proposition, or in their reference to examples of it; and therefore their doctrines are no concern of ours at present. But among those who were really the first to think of the mutual attraction of matter, we cannot help noticing Francis Bacon; for his notions were so far from being chargeable with the looseness and indistinctness to which we have alluded, that he proposed an experiment[20] which was to decide whether the facts were so or not;—whether the gravity of bodies to the earth arose from an attraction of the parts of matter towards each other, or was a tendency towards the centre of the earth. And this experiment is, even to this day, one of the best which can be devised, in order to exhibit the universal gravitation of matter: it consists in the com-

[20] Nov. Org. Works, vol. viii. p. 148.

parison of the rate of going of a clock in a deep mine, and on a high place. Huyghens, in his book "De Causâ Gravitatis," published in 1690, showed that the earth would have an oblate form, in consequence of the action of the centrifugal force; but his reasoning does not suppose gravity to arise from the mutual attraction of the parts of the earth. The influence of the moon upon the tides had long been remarked; but no one had made any progress in truly explaining the mechanism of this influence; and all the analogies to which reference had been made, on this and similar subjects, as magnetic and other attractions, were rather delusive than illustrative, since they represented the attraction as something peculiar in particular bodies, depending upon the nature of each body.

That all such forces, cosmical and terrestrial, were the same single force, and that this was nothing more than the insensible attraction which subsists between one stone and another, was a conception equally bold and grand; and would have been an incomprehensible thought, if the views which we have already explained had not prepared the mind for it. But the preceding steps having disclosed, between all the bodies of the universe, forces of the same kind as those which produce the weight of bodies at the earth, and, therefore, such as exist in every particle of terrestrial matter; it became an obvious question, whether such forces did not also belong to all particles of planetary matter, and whether this

was not, in fact, the whole account of the forces of the solar system. But, supposing this conjecture to be thus suggested, how formidable, on first appearance at least, was the undertaking of verifying it! For if this be so, every finite mass of matter exerts forces which are the result of the infinitely numerous forces of its particles, these forces acting in different directions. It does not appear, at first sight, that the law by which the force is related to the distance, will be the same for the particles as it is for the masses; and, in reality, it is not so, except in special cases. And, again, in the instance of any effect produced by the force of a body, how are we to know whether the force resides in the whole mass as a unit, or in the separate particles? We may reason, as Newton does[21], that the rule which proves gravity to belong universally to the planets, proves it also to belong to their parts; but the mind will not be satisfied with this extension of the rule, except we can find decisive instances, and calculate the effects of both suppositions, under the appropriate conditions. Accordingly, Newton had to solve a new series of problems suggested by this inquiry; and this he did.

These solutions are no less remarkable for the mathematical power which they exhibit, than the other parts of the Principia. The propositions in which it is shown that the law of the inverse square for the particles gives the same law for spherical masses, have that kind of beauty which might well have

[21] B. iii. Prop. 7.

justified their being published for their mathematical elegance alone, even if they had not applied to any real case. Great ingenuity is also employed in other instances, as in the case of spheroids of small eccentricity. And when the amount of the mechanical action of masses of various forms has thus been assigned, the sagacity shown in tracing the results of such action in the solar system is truly admirable; not only the general nature of the effect being pointed out, but its quantity calculated. I speak in particular of the reasonings concerning the figure of the earth, the tides, the precession of the equinoxes, the regression of the nodes of a ring such as Saturn's; and of some effects which, at that time, had not even been ascertained as facts of observation; for instance, the difference of gravity in different latitudes, and the nutation of the earth's axis. It is true, that in most of these cases, Newton's process could be considered only as a rude approximation. In one (the precession) he committed an error, and in all, his means of calculation were insufficient. Indeed these are much more difficult investigations than the problem of three bodies, in which three points act on each other by explicit laws. Up to this day, the resources of modern analysis have been employed upon some of them with very partial success; and the facts, in all of them, required to be accurately ascertained and measured, a process which is not completed even now. Nevertheless the form and nature of the conclusions which Newton did obtain, were such as

to inspire a strong confidence in the competency of his theory to explain all such phenomena as have been spoken of. We shall afterwards have to speak of the labours, undertaken in order to examine the phenomena more exactly, to which the theory gave occasion.

Thus, then, the theory of the universal mutual gravitation of all the particles of matter, according to the law of the inverse square of the distances, was conceived, its consequences calculated, and its results shown to agree with phenomena. It was found that this theory took up all the facts of astronomy as far as they had hitherto been ascertained; while it pointed out an interminable vista of new facts, too minute or too complex for observation alone to disentangle, but capable of being detected when theory had pointed out their laws, and of being used as criteria or confirmations of the truth of the doctrine. For the same reasoning which explained the evection, variation, and annual equation of the moon, showed that there must be many other inequalities besides these; since these resulted from approximate methods of calculation, in which small quantities were neglected. And it was known that, in fact, the inequalities hitherto detected by astronomers did not give the place of the moon with satisfactory accuracy; so that there was room, among these hitherto untractable irregularities, for the additional results of the theory. To work out this comparison was the employment of

the succeeding century; but Newton began it. Thus, at the end of the proposition in which he asserts[22], that "all the lunar motions and their irregularities follow from the principles here stated," he makes the observation which we have just made; and gives, as examples, the different motions of the apogee and nodes, the difference of the change of the eccentricity, and the difference of the moon's variation, according to the different distances of the sun. "But this inequality," he says, "in astronomical calculations, is usually referred to the prosthaphæresis of the moon, and confounded with it."

In Newton's first sketch of the lunar theory, given in the first edition of the Principia in 1687, he had only professed to account for the inequalities in the existing Tables; but he was afterwards led to attempt an improvement of the Tables. This attempt was occasioned by his having seen, when on a casual visit to the Observatory at Greenwich, on September 1, 1694, a list of about one hundred and fifty places of the moon, with the errors, or differences between the observed and computed places, which Flamsteed had drawn up for his own use, and of which he gave Newton a copy. Newton afterwards requested very urgently to be furnished with all the lunar observations which Flamsteed possessed. "If you publish them," he says, "without such a theory to recommend them, they will only be

[22] B. iii. Prop. 22.

thrown into the heap of the observations of former astronomers, till somebody shall arise, that, by perfecting the theory of the moon, shall discover your observations to be exacter than the rest. But when that shall be, God knows; I fear not in your life-time, if I should die before it is done. For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will never be perfected but by somebody who understands the theory of gravity as well or better than I do." And he endeavoured to conciliate the observer by assurances that he should make faithful and honourable mention of him in using the observations. "Indeed," he adds, "all the world knows that I make no observations myself, and therefore I must, of necessity, acknowledge their author; and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown."

The manner of these letters might lead us to suppose that Flamsteed had betrayed some reluctance to grant the request; yet Mr. Baily appears to have shown[23] that Flamsteed transmitted to Newton all

[23] Baily's Flamsteed, App. No. xxvi. p. 151; and Supplement, p. 685. Flamsteed considered Newton's lunar theory merely as an intended improvement of the Tables, and did not share the enthusiasm of Halley and others, who justly admired it as a great physical discovery. But Mr. Baily has very clearly proved that the importance of the Greenwich observations to the Newtonian lunar theory had nothing to do with the unhappy disputes respecting the publication of the observations which afterwards occurred.

his lunar observations. The reformation of the tables turned out more difficult than had been foreseen, and did not lead to any very great improvement till a later period.

Reflections on the Discovery.—Such, then, is the great Newtonian induction of universal gravitation, and such its history. It is indisputably and incomparably the greatest scientific discovery ever made, whether we look at the advance which it involved, the extent of the truth disclosed, or the fundamental and satisfactory nature of this truth. As to the first point, we may observe that any one of the five steps into which we have separated the doctrine, would, of itself, have been considered as an important advance;—would have conferred distinction on the persons who made it, and the time to which it belonged. All the five steps made at once, formed not a leap, but a flight,—not an improvement merely, but a metamorphosis,—not an epoch, but a termination. Astronomy passed at once from its boyhood to mature manhood. Again, with regard to the extent of the truth, we obtain as wide a generalisation as our physical knowledge admits, when we learn that every particle of matter, in all times, places, and circumstances, attracts every other particle in the universe by one common law of action. And by saying that the truth was of a fundamental and satisfactory nature, I mean that it assigned not a rule merely, but a cause, for the heavenly motions; and that kind of cause which most eminently and pecu-

liarly we distinctly and thoroughly conceive, namely, mechanical force. Kepler's laws were merely *formal* rules, governing the celestial motions according to the relations of space, time, and number; Newton's was a *causal* law, referring these motions to mechanical reasons. It is no doubt conceivable that future discoveries may both extend and further explain Newton's doctrines;—may make gravitation a case of some wider law, and may disclose something of the mode in which it operates; questions with which Newton himself struggled. But, in the mean time, few persons will dispute, that both in generality and in profundity, both in width and depth, Newton's theory is altogether without a rival or neighbour[24].

[24] The value and nature of this step have long been generally acknowledged wherever science is cultivated. Yet it would appear that there is, in one part of Europe, a school of philosophers who contest the merit of this part of Newton's discoveries. "Kepler," says a celebrated German metaphysician*, "discovered the laws of free motion; a discovery of immortal glory. It has since been the fashion to say that Newton first found out the proof of these rules. It has seldom happened that the glory of the first discoverer has been more unjustly transferred to another person." It may appear strange that any one in the present day should hold such language; but if we examine the reasons which this author gives, they will be found, I think, to amount to this; that his mind is in the condition in which Kepler's was; and that the whole range of mechanical ideas and modes of conception which made the transition from Kepler to Newton possible, are extraneous to the domain of his philosophy. Even this author, however, if I understand him rightly, recognises Newton as the author of the doctrine of Perturbations.

* Hegel, Encyclopædia, § 270.

The requisite conditions of such a discovery in the mind of its author were, in this as in other cases, the idea, and its comparison with facts;—the conception of the law, and the moulding this conception in such a form as to correspond with known realities. The idea of mechanical force as the cause of the celestial motions, had, as we have seen, been for some time growing up in men's minds;—had gone on becoming more distinct and more general; and had, in some persons, approached the form in which it was entertained by Newton. Still, in the mere conception of universal gravitation, Newton must have gone far beyond his predecessors and contemporaries, both in generality and distinctness; and in the inventiveness and sagacity with which he traced the consequences of this conception, he was, as we have shown, without a rival, and almost without a second. As to the facts which he had to include in his law, they had been accumulating from the very birth of astronomy; but those which he had more peculiarly to take hold of, were the facts of the planetary motions as given by Kepler, and those of the moon's motions as given by Tycho Brahe and Jeremy Horrox.

We find here occasion to make a remark which is important in its bearing on the nature of progressive science. What Newton thus used and referred to as *facts*, were the *laws* which his predecessors had established. What Kepler and

Horrox had put forth as "theories," were now established truths, fit to be used in the construction of other theories. It is in this manner that one theory is built upon another;—that we rise from particulars to generals, and from one generalisation to another;—that we have, in short, successive steps of induction. As Newton's laws assumed Kepler's, Kepler's laws assumed as facts the results of the planetary theory of Ptolemy; and thus the theories of each generation in the scientific world are (when thoroughly verified and established,) the facts of the next generation. Newton's theory is the circle of generalisation which includes all the others;—the highest point of the inductive ascent;—the catastrophe of the philosophic drama to which Plato had prologized;—the point to which men's minds had been journeying for two thousand years.

Character of Newton.—It is not easy to anatomise the constitution and the operations of the mind which makes such an advance in knowledge. Yet we may observe that there must exist in it, in an eminent degree, the elements which compose the mathematical talent. It must possess distinctness of intuition, tenacity and facility in tracing logical connexion, fertility of invention, and a strong tendency to generalisation. It is easy to discover indications of these characteristics in Newton. The distinctness of his intuitions of space, and we may add of force also, was seen in the amusements of his youth; in his constructing clocks and mills, carts

and dials, as well as the facility with which he mastered geometry. This fondness for handicraft employments, and for making models and machines, appears to be a common prelude of excellence in physical science[23]; probably on this very account, that it arises from the distinctness of intuitive power with which the child conceives the shapes and the working of such material combinations. Newton's inventive power appears in the number and variety of the mathematical artifices and combinations which he devised, and of which his books are full. If we conceive the operation of the inventive faculty in the only way in which it appears possible to conceive it;—that while some hidden source supplies a rapid stream of possible suggestions, the mind is on the watch to seize and detain any one of these which will suit the case in hand, allowing the rest to pass by and be forgotten;—we shall see what extraordinary fertility of mind is implied by so many successful efforts; what an innumerable host of thoughts must have been produced, to supply so many that deserved to be selected. And since the selection is performed by tracing the consequences of each suggestion, so as to compare them with the requisite conditions, we see also what rapidity and certainty in drawing conclusions the mind must possess as a talent, and what watchfulness and patience as a habit.

The hidden fountain of our unbidden thoughts is for

[23] As in Galileo, Hooke, Huyghens, and others.

us a mystery; and we have, in our consciousness, no standard by which we can measure our own talents; but our acts and habits are something of which we are conscious; and we can understand, therefore, how it was that Newton could not admit that there was any difference between himself and other men, except in his possession of such habits as we have mentioned, perseverance and vigilance. When he was asked how he made his discoveries, he answered, "by always thinking about them;" and at another time, he declared that if he had done anything, it was due to nothing but industry and patient thought: "I keep the subject of my inquiry constantly before me, and wait till the first dawning opens gradually, by little and little, into a full and clear light." No better account can be given of the nature of the mental *effort* which gives to the philosopher the full benefit of his powers; but the natural *powers* of men's minds are not on that account the less different. There are many who might wait through ages of darkness without being visited by any dawn.

The habit to which Newton thus, in some sense, owed his discoveries, this constant attention to the rising thought, and developement of its results in every direction, necessarily engaged and absorbed his spirit, and made him inattentive and almost insensible to external impressions and common impulses. The stories which are told of his extreme absence of mind, probably refer to the two years during which he was composing his Principia, and thus following

out a train of reasoning the most fertile, the most complex, and the most important, which any philosopher had ever had to deal with. The magnificent and striking questions which, during this period, he must have had daily rising before him; the perpetual succession of difficult problems of which the solution was necessary to his great object; may well have entirely occupied and possessed him. "He existed only to calculate and to think[26]." Often, lost in meditation, he knew not what he did, and his mind appeared to have quite forgotten its connexion with the body. His servant reported that, in rising in a morning, he frequently sat a large portion of the day, half-dressed, on the side of his bed; and that his meals waited on the table for hours before he came to take them. Even with his transcendent powers, to do what he did, was almost irreconcileable with the common conditions of human life; and required the utmost devotion of thought, energy of effort, and steadiness of will,—the strongest character, as well as the highest endowments, which belong to man.

Newton has been so universally considered as the greatest example of a natural philosopher, that his moral qualities, as well as his intellect, have been referred to as models of the philosophical character; and those who love to think that great talents are naturally associated with virtue, have always dwelt

[26] Biot.

with pleasure upon the views given of Newton by his contemporaries; for they have uniformly represented him as candid and humble, mild and good. We may take as an example of the impressions prevalent about him in his own time, the expressions of Thomson, in the Poem on his Death[27].

> Say ye who best can tell, ye happy few,
> Who saw him in the softest lights of life,
> All unwithheld, indulging to his friends
> The vast unborrowed treasures of his mind,
> Oh, speak the wondrous man! how mild, how calm,
> How greatly humble, how divinely good,
> How firm established on eternal truth!
> Fervent in doing well, with every nerve
> Still pressing on, forgetful of the past,
> And panting for perfection; far above
> Those little cares and visionary joys
> That so perplex the fond impassioned heart
> Of ever-cheated, ever-trusting man.

[27] In the same strain we find the general voice of the time. For instance, one of Loggan's "Views of Cambridge" is dedicated "Isaaco Newtono . . Mathematico, Physico, Chymico consummatissimo; nec minus suavitate morum et candore animi . . . spectabili."

In opposition to the general current of such testimony, we have the complaints of Flamsteed, who ascribes to Newton angry language and harsh conduct in the matter of the publication of the Greenwich Observations. That Flamsteed himself was weak and prejudiced, sore and angry, is very clear; Newton and others, acting officially, thought themselves bound to disregard his wishes; and it seems probable that Flamsteed, in thinking that Newton behaved with such want of temper as he describes, saw him distorted through the stormy medium of his own feelings.

CHAPTER III.

SEQUEL TO THE EPOCH OF NEWTON.—RECEPTION OF THE NEWTONIAN THEORY.

Sect. 1.—*General Remarks.*

THE doctrine of universal gravitation, like other great steps in science, required a certain time to make its way into men's minds; and had to be confirmed, illustrated, and completed, by the labours of succeeding philosophers. As the discovery itself was great beyond former example, the features of the natural sequel to the discovery were also on a gigantic scale; and many vast and laborious trains of research, each of which might, in itself, be considered as forming a wide science, and several of which have occupied many profound and zealous inquirers from that time to our own day, come before us as parts only of the verification of Newton's theory. Almost everything that has been done, and is doing, in astronomy, falls inevitably under this description; and it is only when the astronomer travels to the very limits of his vast field of labour, that he falls in with phenomena which do not acknowledge the jurisdiction of the Newtonian legis-

lation. We must give some account of the events of this part of the history of astronomy; but our narrative must necessarily be extremely brief and imperfect; for the subject is most large and copious, and our limits are fixed and narrow. We have to do with the history of discoveries, only so far as it illustrates their philosophy. And though the astronomical discoveries of the last century are by no means poor, even in interest of this kind, the generalisations which they involve are far less important for our object, in consequence of being included in a previous generalisation. Newton shines out so brightly, that all who follow seem faint and dim. It is not precisely the case which the poet describes;

> As in a theatre the eyes of men,
> After some well-graced actor leaves the stage,
> Are idly bent on him that enters next,
> Thinking his prattle to be tedious:

but our eyes are at least less intently bent on the astronomers who succeeded, and we attend to their communications with less curiosity, because we know the end, if not the course, of their story; we know that their speeches have all closed with Newton's sublime declaration, asserted in some new form.

Still, however, the account of the verification and extension of any great discovery, is a highly-important part of its history. In this instance it is most important; both from the weight and dignity of the theory concerned, and the ingenuity and extent of the methods employed: and, of course, so long as

the Newtonian theory still required verification, the question of the truth or falsehood of such a grand system of doctrines could not but excite the most intense curiosity. In what I have said, I am very far from wishing to depreciate the value of the achievements of modern astronomers, but it is essential to my purpose to mark the subordination of narrower to wider truths,—the different character and import of the labours of those who come before and after the promulgation of a master-truth. With this warning I now proceed to my narrative.

Sect. 2.—Reception of the Newtonian Theory in England.

THERE appears to be a popular persuasion that great discoveries are usually received with a prejudiced and contentious opposition, and the authors of them neglected or persecuted. The reverse of this was certainly the case in England with regard to the discoveries of Newton. As we have already seen, even before they were published, they were proclaimed by Halley to be something of transcendent value; and from the moment of their appearance, they rapidly made their way from one class of thinkers to another, nearly as fast as the nature of men's intellectual capacity allows. Halley, Wren, and all the leading members of the Royal Society, appear to have embraced the system immediately and zealously. Men whose pursuits had lain rather

in literature than in science, and who had not the knowledge and habits of mind which the strict study of the system required, adopted, on the credit of their mathematical friends, the highest estimation of the Principia, and a strong regard for its author, as Evelyn, Locke, and Pepys. Only five years after the publication, the principles of the work were referred to from the pulpit, as so incontestably proved that they might be made the basis of a theological argument. This was done by Dr. Bentley, when he preached the Boyle's Lectures in London, in 1692. Newton himself, from the time when his work appeared, is never mentioned except in terms of profound admiration; as, for instance, when he is called by Dr. Bentley, in his sermon[1], "That very excellent and divine theorist, Mr. Isaac Newton." It appears to have been soon suggested, that the Government ought to provide in some way for a person who was so great an honour to the nation. Some delay took place with regard to this; but, in 1695 his friend Mr. Montague, afterwards Earl of Halifax, made him Warden of the Mint; and, in 1699, he succeeded to the higher office of Master of the Mint, a situation worth 1200*l.* or 1500*l.* a year, which he filled to the end of his life. In 1703, he became President of the Royal Society, and was annually re-elected to this office during the remaining twenty-five years of his life. In 1705, he was knighted

[1] Serm. vii. 221.

in the Master's Lodge, at Trinity College, by Queen Anne, then on a visit to the University of Cambridge. After the accession of George the First, Newton's conversation was frequently sought by the Princess, afterwards Queen Caroline, who had a taste for speculative studies, and was often heard to declare in public, that she thought herself fortunate in living at a time which enabled her to enjoy the society of so great a genius. His fame, and the respect paid him, went on increasing to the end of his life; and when, in 1727, full of years and glory, his earthly career was ended, his death was mourned as a national calamity, with the forms usually confined to royalty. His body lay in state in the Jerusalem Chamber; his pall was borne by the first nobles of the land; and his earthly remains were deposited in the centre of Westminster Abbey, in the midst of the memorials of the greatest and wisest men whom England has produced.

It cannot be superfluous to say a word or two on the reception of his philosophy in the universities of England. These are often represented as places where bigotry and ignorance resist, as long as it is possible to resist, the invasion of new truths. We cannot doubt that such opinions have prevailed extensively, when we find an intelligent and generally temperate writer, like the late Professor Playfair of Edinburgh, so far possessed by them, as to be incapable of seeing, or interpreting, in any other way, any facts respecting Oxford and Cambridge.

Yet, notwithstanding these opinions, it will be found that, in the English universities, new views, whether in science or in other subjects, have been introduced as soon as they were clearly established;—that they have been diffused from the few to the many more rapidly there than elsewhere occurs;—and that from these points, the lights of newly discovered truths have most usually spread over the land. In most instances undoubtedly there has been something of a struggle on such occasions, between the old and the new opinions. Few men's minds can at once shake off a familiar and consistent system of doctrines, and adopt a novel and strange set of principles as soon as presented: but all can see that one change produces many, and that change, in itself, is a source of inconvenience and danger. In the case of the admission of the Newtonian opinions into Cambridge and Oxford, however, there are no traces even of a struggle. Cartesianism had never struck its roots deep in this country; that is, the peculiar hypotheses of Descartes. The Cartesian books, such, for instance, as that of Rohault, were indeed in use; and with good reason; for they contained by far the best treatises on most of the physical sciences, such as mechanics, hydrostatics, optics, and formal astronomy, which could then be found. But I do not conceive that the vortices were ever dwelt upon as a matter of importance in our academic teaching. At any rate, if they were brought among us, they were soon dissipated. Newton's college, and his

university, exulted in his fame, and did their utmost to honour and aid him. He was exempted by the king from the obligation of taking orders, under which the fellows of Trinity College in general are; by his college he was relieved from all offices which might interfere, however slightly, with his studious employments, though he resided within the walls of the society thirty-five years, almost without the interruption of a month[a]. By the University he was elected their representative in parliament in 1688, and again in 1701; and though he was rejected in the dissolution in 1705, those who opposed him acknowledged him[b] to be "the glory of the University and nation," but considered the question as a political one, and Newton as sent "to tempt them from their duty, by the great and just veneration they had for him." Instruments and other memorials, valued because they belonged to him, are still preserved in his college, along with the tradition of the chambers which he occupied.

The most active and powerful minds at Cambridge became at once disciples and followers of Newton. Samuel Clarke, afterwards his friend, defended in the public schools a thesis taken from his philosophy, as early as 1694; and in 1697 published an edition

[a] I infer this from the fact that his name is nowhere found on the college books, as appointed to any of the offices which usually pass down the list of resident fellows in rotation. The constancy of his residence in college appears from the *exit* and *rediit* book of that time, which is still preserved.

[b] Styan Thurlby's pamphlet.

of Rohault's Physics, with notes, in which Newton is frequently referred to with expressions of profound respect, though the leading doctrines of the Principia are not introduced till a later edition, in 1703. In 1699 Bentley, whom we have already mentioned as a Newtonian, became master of Trinity College; and in the same year, Whiston, another of Newton's disciples was appointed his deputy as professor of mathematics. Whiston delivered the Newtonian doctrines, both from the professor's chair, and in works written for the use of the University; yet it is remakable that a taunt respecting the late introduction of the Newtonian system into the Cambridge course of education, has been founded on some peevish expressions which he uses in his Memoirs, written at a period when, having incurred expulsion from his professorship and the University, he was naturally querulous and jaundiced in his views. In 1709-10 Dr. Laughton, who was tutor in Clare Hall, procured himself to be appointed moderator of the University disputations, in order to promote the diffusion of the new mathematical doctrines. By this time the first edition of the Principia was become rare, and fetched a great price. Bentley urged Newton to publish a new one; and Cotes, by far the first, at that time, of the mathematicians of Cambridge, undertook to superintend the printing, and the edition was accordingly published in 1713.

At Oxford, David Gregory and Halley, both zealous and distinguished disciples of Newton, obtained

the Savilian professorships of astronomy and geometry in 1691 and 1703; and in 1704 or 5, Keill publicly taught the Newtonian philosophy by experiment. In the Scotch Universities, James Gregory, who was professor at St. Andrew's, accepted the Newtonian philosophy with singular alacrity, for he is said[4], as early as 1690, to have printed a thesis containing, in twenty-two positions, a compend of Newton's Principia. David Gregory, his brother, was, before he removed to Oxford, professor at Edinburgh; and would no doubt introduce the new discoveries there. The general diffusion of these opinions in England took place, not only by means of books, but through the labours of various experimental lecturers, like Desaguliers, who removed from Oxford to London in 1713; when he informs us[5], that "he found the Newtonian philosophy generally received among persons of all ranks and professions, and even among the ladies by the help of experiments."

We might easily trace in our literature indications of the gradual progress of the Newtonian doctrines. For instance, in the earlier editions of Pope's Dunciad, this couplet occurred, in the description of the effects of the reign of Dulness:—

> Philosophy, that reached the heavens before,
> Shrinks to her hidden cause, and is no more.

"And this," says his editor, Warburton, "was in-

[4] Hutton's Dict., art. D. Gregory. [5] Desag. Pref.

tended as a censure on the Newtonian philosophy. For the poet had been misled by the prejudices of foreigners, as if that philosophy had recurred to the occult qualities of Aristotle. This was the idea he received of it from a man educated much abroad, who had read everything, but everything superficially[6]. When I hinted to him how he had been imposed upon, he changed the lines with great pleasure into a compliment (as they now stand) on that divine genius, and a satire on that very folly by which he himself had been misled." In 1743 it was printed,

> Philosophy, that leaned on heaven before,
> Shrinks to her second cause, and is no more.

The Newtonians repelled the charge of dealing in occult causes[7]; and, referring gravity to the will of the Deity, as the First Cause, assumed a superiority over those whose philosophy rested in second causes.

To the willing reception of the Newtonian theory by the English astronomers, there is only one conspicuous exception; which is, however, one of some note, being no other than Flamsteed, the Astronomer Royal, a most laborious and exact observer. Flamsteed at first listened with complacency to the promises which the new doctrines held forth, and appeared willing to assist Newton, and to receive

[6] I presume Bolingbroke is here meant.

[7] See Cotes's Preface to the Principia.

assistance from him. But he soon quarrelled with Newton's theory, as we have seen that he did with the author, and declares to his correspondent[8], "I have determined to lay these crotchets of Sir Isaac Newton's wholly aside." We need not, however, find any difficulty in this, if we recollect that Flamsteed, though a good observer, was no philosopher;—could never understand by a theory anything more than a formula which should predict results;—and was incapable of comprehending the object of Newton's theory, which was to assign causes as well as rules, and to satisfy the conditions of mechanics as well as of geometry.

Sect. 2.—*Reception of the Newtonian Theory abroad.*

THE reception of the Newtonian theory on the Continent, was much more tardy and unwilling than in its native island. Even those whose mathematical attainments most fitted them to appreciate its proofs, were prevented by some peculiarity of view from adopting it as a system; as Leibnitz, Bernoulli, Huyghens; who all clung to one modification or other of the system of vortices. In France, the Cartesian system had obtained a wide and popular reception, having been recommended by Fontenelle with the graces of his style; and its empire was so firm and well established in that country, that it

[8] Account of Flamsteed, &c., p. 309.

resisted for a long time the pressure of Newtonian arguments. Indeed, the Newtonian opinions had scarcely any disciples in France, till Voltaire asserted their claims, on his return from England in 1728: till then, as he himself says, there were not twenty Newtonians out of England.

The hold which the philosophy of Descartes had upon the minds of his countrymen is, perhaps, not surprising. He really had the merit, a great one in the history of science, of having completely overturned the Aristotelian system, and introduced the philosophy of matter and motion. In all branches of mixed mathematics, as we have already said, his followers were the best guides who had yet appeared. His hypothesis of vortices, as an explanation of the celestial motions, had an apparent advantage over the Newtonian doctrine, in this respect;—that it referred effects to the most intelligible, or at least most familiar kinds, of mechanical causation, namely, pressure and impulse. And above all, the system was acceptable to most minds, in consequence of being, as was pretended, deduced from a few simple principles by necessary consequence; and of being also directly connected with metaphysical and theological speculations. We may add, that it was modified by its mathematical adherents in such a way as to remove most of the objections to it. A vortex revolving about a centre could be constructed, or at least it was supposed that it could be constructed, so as to produce a tendency of bodies to

the centre. In all cases, therefore, where a central force acted, a vortex was supposed; but in reasoning out the results of this hypothesis, it was easy to leave out of sight all other effects of the vortex, and to consider only the central force: and when this was done, the Cartesian mathematician could apply to his problems a mechanical principle of some degree of consistency. This reflection will, in some degree, account for what at first seems so strange;—the fact, that the language of the French mathematicians is Cartesian, for almost half a century after the publication of the Principia of Newton.

There was, however, a controversy between the two opinions going on all this time, and every day showed the insurmountable difficulties under which the Cartesians laboured. Newton, in the Principia, had inserted a series of propositions, the object of which was to prove, that the machinery of vortices could not be accommodated to one part of the celestial phenomena, without contradicting another part. A more obvious difficulty was the case of gravity of the earth; if this force arose, as Descartes asserted, from the rotation of the earth's vortex about its axis, it ought to tend directly to the axis, and not to the centre. The asserters of vortices often tried their ill in remedying this vice in the hypothesis, but never with much success. Huyghens supposed the ıereal matter of the vortices to revolve about the re in all directions; Perrault made the strata of vortex increase in velocity of rotation as they

recede from the centre; Saurin maintained that the circumambient resistance which comprises the vortex will produce a pressure passing through the centre. The elliptic form of the orbits of the planets was another difficulty. Descartes had supposed the vortices themselves to be oval; but others, as John Bernoulli, contrived ways of having elliptical motion in a circular vortex.

The mathematical prize-questions proposed by the French Academy, naturally brought the two sets of opinions into conflict. The Cartesian Memoir of John Bernoulli, to which we have just referred, was the one which gained the prize in 1730. It not unfrequently happened that the Academy, as if desirous to show its impartiality, divided the prize between Cartesians and Newtonians. Thus in 1734, the question being, the cause of the inclination of the orbits of the planets, the prize was shared between John Bernoulli, whose Memoir was founded on the system of vortices, and his son Daniel, who was a Newtonian. The last act of homage of this kind to the Cartesian system was performed in 1740, when the prize on the question of the tides was distributed between Daniel Bernoulli, Euler, Maclaurin, and Cavallieri; the last of whom had tried to amend and patch up the Cartesian hypothesis on this subject.

Thus the Newtonian system was not adopted in France till the Cartesian generation had died off;

Fontenelle, who was secretary to the Academy of Sciences, and who lived till 1756, died a Cartesian. There were exceptions; for instance, Delisle, an astronomer who was selected by Peter the Great of Russia, to found the Academy of St. Petersburg; who visited England in 1724, and to whom Newton then gave his picture, and Halley his Tables. But in general, during the interval, that country and this had a national difference of creed on physical subjects. Voltaire, who visited England in 1727, notices this difference in his lively manner. "A Frenchman who arrives in London, finds a great alteration in philosophy, as in other things. He left the world full, he finds it empty. At Paris you see the universe composed of vortices of subtile matter, at London we see nothing of the kind. With you it is the pressure of the moon which causes the tides of the sea, in England it is the sea which gravitates towards the moon; so that when you think the moon ought to give us high water, these gentlemen believe that you ought to have low water; which unfortunately we cannot test by experience; for in order to do that, we should have examined the moon and the tides at the moment of the creation. You will observe also that the sun, which in France has nothing to do with the business, here comes in for a quarter of it. Among you Cartesians, all is done by an impulsion which one does not well understand; with the Newtonians, it is done by an attrac-

tion of which we know the cause no better. At Paris you fancy the earth shaped like a melon, at London it is flattened on the two sides."

It was Voltaire himself, as we have said, who was mainly instrumental in giving the Newtonian doctrines currency in France. He was at first refused permission to print his "Elements of the Newtonian Philosophy," by the chancellor, D'Aguesseaux, who was a Cartesian; but after the appearance of this work in 1738, and of other writings by him on the same subject, the Cartesian edifice, already without real support or consistency, crumbled to pieces and disappeared. The first Memoir in the Transactions of the French Academy in which the doctrine of central forces is applied to the solar system, is one by the Chevalier de Louville in 1720, "On the Construction and Theory of Tables of the Sun." In this, however, the mode of explaining the motions of the planets by means of an original impulse and an attractive force is attributed to Kepler, not to Newton. The first Memoir which refers to the universal gravitation of matter is by Maupertuis, in 1736. Nor was Newton unknown or despised in France till this time. In 1699 he was admitted one of the very small number of foreign associates of the French Academy of Sciences. Even Fontenelle, who, as we have said, never adopted his opinions, spoke of him in a worthy manner, in the *Eloge* which he composed on the occasion of his death. At a much earlier period too, Fontenelle

did homage to his fame. The following passage refers, I presume, to Newton. In the *History* of the Academy for 1708, which is written by the secretary, he says[9], in referring to the difficulty which the comets occasion in the Cartesian hypothesis: "We might relieve ourselves at once from all the embarrassment which arises from the directions of these motions, by suppressing, as has been done *by one of the greatest geniuses of the age*, all this immense fluid matter, which we commonly suppose between the planets, and conceiving them suspended in a perfect void."

Comets, as the above passage implies, were a kind of artillery which the Cartesian *plenum* could not resist. When it appeared that the paths of such wanderers traversed the vortices in all directions, it was impossible to maintain that these imaginary currents governed the movements of bodies immersed in them; and the mechanism ceased to have any real efficacy. Both these phenomena of comets, and many others, became objects of a stronger and more general interest, in consequence of the controversy between the rival parties; and thus the prevalence of the Cartesian system did not seriously impede the progress of sound knowledge. In some cases, no doubt, it made men unwilling to receive the truth, as in the instance of the deviation of the comets from the zodiacal motion; and again, when

[9] Hist. Ac. Sc. 1708, p. 103.

Römer discovered that light was not instantaneously propagated. But it encouraged observation and calculation, and thus forwarded the verification and extension of the Newtonian system; of which process we must now consider some of the incidents

CHAPTER IV.

SEQUEL TO THE EPOCH OF NEWTON, CONTINUED.—VERIFICATION AND COMPLETION OF THE NEWTONIAN THEORY.

Sect. 1.—*Division of the Subject.*

THE verification of the law of universal gravitation as the governing principle of all cosmical phenomena, led, as we have already stated, to a number of different lines of research, all long and difficult. Of these we may treat successively, the motions of the moon, of the sun, of the planets, of the satellites, of comets; we may also consider separately the secular inequalities, which at first sight appear to follow a different law from the other changes; we may then speak of the results of the principle as they affect this earth, in its form, in the amount of gravity, and in the phenomena of the tides. Each of these subjects has lent its aid to confirm the general law; but in each the confirmation has had its peculiar difficulties, and has its separate history. Our sketch of this history must be very rapid, for our aim is only to show what is the kind and course of the confirmation which such a theory demands and receives.

For the same reason we pass over many events of this period which are highly important in the history

of astronomy. But they have lost much of their interest for us, and even for common readers, because they are of a class with which we are already familiar, truths included in more general truths to which our eyes now most readily turn. Thus, the discovery of new satellites and planets is but a repetition of what was done by Galileo: the determination of their nodes and apses, the reduction of their motions to the law of the ellipse, is but a fresh exemplification of the discoveries of Kepler. Otherwise, the formation of tables of the satellites of Jupiter and Saturn, the discovery of the eccentricities of the orbits, the motions of the nodes and apses, by Cassini, Halley, and others, would rank with the great achievements in astronomy. Newton's peculiar advance in the tables of the celestial motions is the introduction of perturbations. To these motions, so affected, we now proceed.

Sect. 2.—Application of the Newtonian Theory to the Moon.

THE motions of the moon may be first spoken of, as the most obvious and the most important of the applications of the Newtonian theory. The verification of such a theory consists, as we have seen in previous cases, in the construction of tables from the theory, and the comparison of these with observation. The advancement of astronomy would alone have been a sufficient motive for this labour; but

there were other reasons which urged it on with a stronger impulse. A perfect lunar theory, if the theory could be perfected, promised to supply a method of finding the longitude of any place; and thus the verification of a theory which professed to be complete in its foundations, was identified with an object of immediate practical use to navigators and geographers, and of vast acknowledged value. A good method for the near discovery of the longitude had been estimated by nations and princes at large sums of money. The Dutch were willing to tempt Galileo to this task by the offer of a chain of gold: Philip the Third of Spain had promised a reward for this object still earlier[1]; the parliament of England, in 1714, proposed a recompense of 20,000*l.* sterling; the Regent Duke of Orléans, two years afterwards, offered 100,000 francs for the same purpose. These prizes, added to the love of truth and of fame, kept this object constantly before the eyes of mathematicians, during the first half of the last century.

If the tables could be so constructed as to represent the moon's real place in the heavens with extreme precision, as it would be seen from a standard observatory, the observation of her apparent place, as seen from any other point of the earth's surface, would enable the observer to find his longitude. The motions of the moon had hitherto so ill agreed with the best tables, that this method failed altogether.

[1] Del. A. M. i. 39, 66.

Newton had discovered the ground of this want of agreement. He had shown that the same force which produces the evection, variation, and annual equation, must produce also a long series of other inequalities, of various magnitudes and cycles, which perpetually drag the moon before or behind the place where she would be sought by an astronomer who knew only of those principal and notorious inequalities. But to calculate and apply the new inequalities, was no slight undertaking.

In the first edition of the Principia in 1687, Newton had not given any calculations of new inequalities affecting the longitude of the moon. But in David Gregory's "Elements of Physical and Geometrical Astronomy," published in 1702, is inserted[2] "Newton's Lunar Theory as applied by him to Practice;" in which the great discoverer has given the results of his calculations of eight of the lunar equations, their quantities, epochs, and periods. These calculations were for a long period the basis of new Tables of the Moon, which were published by various persons[3]; as by De L'Isle in 1715 or 1716, Grammatici at Ingoldstadt in 1726, Wright in 1732, Angelo Capelli at Venice in 1733, Dunthorne at Cambridge in 1739.

We have seen how solicitous Newton himself was to bring about an accordance between his Tables and Flamsteed's observations; and how much the delay

[2] p. 332.

[3] Lalande, 1457.

in the publication of the observations was regretted and resisted by the most zealous English men of science. Flamsteed had given Tables of the Moon upon Horrox's theory in 1681, and wished to improve them; and though, as we have seen, he would not, or could not, accept Newton's doctrines in their whole extent, Newton communicated his theory to the observer in the shape in which he could understand it and use it[4]: and Flamsteed employed these directions in constructing new Lunar Tables, which he called his Theory[5]. These Tables were not published till long after his death, by Le Monnier at Paris in 1746. They are said, by Lalande[6], not to differ much from Halley's. Halley's Tables of the Moon were printed in 1719 or 1720, but not published till after his death in 1749. They had been founded on Flamsteed's observations and his own; and when, in 1720, Halley succeeded Flamsteed in the post of Astronomer Royal at Greenwich, and conceived that he had the means of much improving what he had done before, he began by printing what he had already executed.

But Halley had long proposed a method, different from that of Newton, but marked by great ingenuity, for amending the Lunar Tables. He proposed to do this by the use of a cycle, which we have mentioned as one of the earliest discoveries in astronomy; —the period of 223 lunations, or eighteen years and

[4] Account of Flamsteed, p. 72.
[5] p. 211.
[6] Lal. 1459.

eleven days, the Chaldean Saros. This period was anciently used for predicting the eclipses of the sun and moon; for those eclipses which happen during this period, are repeated again in the same order, and with nearly the same circumstances, after the expiration of one such period and the commencement of a second. The reason of this is, that at the end of such a cycle, the moon is in nearly the same position with respect to the sun, her nodes, and her apogee, as she was at first; and is only a few degrees distant from the same part of the heavens. But on the strength of this consideration, Halley conjectured that all the irregularities of the moon's motion, however complex they may be, would recur after such an interval; and that therefore, if the requisite corrections were determined by observation for one such period, we might by means of them give accuracy to the tables for all succeeding periods. This idea occurred to him before he was acquainted with Newton's views[7]. After the lunar theory of the Principia had appeared, he could not help seeing that the idea was confirmed; for the inequalities of the moon's motion, which arise from the attraction of the sun, will depend on her positions with regard to the sun, the apogee, and the node; and therefore, however numerous, will recur when these positions recur.

Halley announced, in 1691[8], his intention of fol-

[7] Phil. Trans. 1731, p. 188.

[8] Phil. Tr. p. 536.

lowing this idea into practice; in a paper in which he corrected the text of three passages in Pliny, in which this period is mentioned, and from which it is sometimes called the Plinian period. In 1710, in the preface to a new edition of Street's Caroline Tables, he stated that he had already confirmed it to a considerable extent[9]. And even after Newton's theory had been applied, he still resolved to use his cycle as a means of obtaining further accuracy. On succeeding to the Observatory at Greenwich in 1720, he was further delayed by finding that the instruments had belonged to Flamsteed, and were removed by his executors. "And this," he says[10], "was the more grievous to me, on account of my advanced age, being then in my sixty-fourth year; which put me past all hopes of ever living to see a complete period of eighteen years' observation. But, thanks to God, he has been pleased hitherto (in 1731) to afford me sufficient health and strength to execute my office, in all its parts, with my own hands and eyes, without any assistance or interruption, during one whole period of the moon's apogee, which period is performed in somewhat less than nine years." He found the agreement very remarkable, and conceived hopes of attaining the great object, of finding the longitude with the requisite degree of exactness; nor did he give up his labours on this subject till he had completed his Plinian period in 1739.

[9] Phil. Tr. 1731, p. 187.

[10] Ib. p. 193.

The accuracy with which Halley conceived himself able to predict the moon's place[11] was within two minutes of space, or one-fifteenth of the breadth of the moon herself. The accuracy required for obtaining the national reward was considerably greater. Le Monnier pursued the idea of Halley[12]. But before Halley's method had been completed, it was superseded by the more direct prosecution of Newton's views.

We have already remarked, in the history of analytical mechanics, that in the lunar theory, considered as one of the cases of the problem of three bodies, no advance was made beyond what Newton had done, till mathematicians threw aside the Newtonian artifices, and applied the newly-developed generalisations of the analytical method. The first great apparent deficiency in the agreement of the law of universal gravitation with astronomical observation, was removed by Clairaut's improved approximation in 1750; yet not till it had caused so much disquietude, that Clairaut himself had suggested a modification of the law of abstraction; and it was only in tracing the consequences of this suggestion, that he found the Newtonian law of the inverse square to be that which, when rightly developed, agreed with the facts. Euler[13] solved the problem by the aid of his analysis in 1745, and published Tables of the Moon in 1746. His tables were not very accurate at first[14];

[11] P. T. 1731, p. 195.
[12] Bailly, A. M. c. 131.
[13] Lal. 1460.
[14] Bradley's Correspondence.

but he, D'Alembert, and Clairaut, continued to labour at this object, and the two latter published Tables of the Moon in 1754[15]. Finally, Tobias Mayer, an astronomer of Göttingen, having compared Euler's tables with observations, corrected them so successfully, that in 1753 he published Tables of the Moon, which really did possess the accuracy which Halley only flattered himself that he had attained. Mayer's success in his first tables encouraged him to make them still more perfect. He applied himself to the mechanical theory of the moon's orbit; corrected all the co-efficients of the series by a great number of observations; and, in 1755, sent his new tables to London as worthy to claim the prize offered for the longitude. He died soon after (in 1762,) at the early age of thirty-nine, worn out by his incessant labours; and his widow sent to London a copy of his tables with additional corrections. These tables were committed to Bradley, then Astronomer Royal, in order to be compared with observation. Bradley laboured at this task with unremitting zeal and industry, having himself long entertained hopes that the lunar method of finding the longitude might be brought into general use. He and his assistant, Gael Morris, introduced corrections into Mayer's tables of 1755. In his report of 1756, he says[16], that he did not find any difference so great as a minute and a quarter, and

[15] Lal. 1460.

[16] Bradley's Mem. p. xcviii.

in 1760, he adds, that this deviation had been further diminished by his corrections. It is not foreign to our purpose to observe the great labour which this verification required. Not less than 1220 observations, and long calculations founded upon each, were employed. The accuracy which Mayer's Tables possessed was considered to entitle them to a part of the parliamentary reward; they were printed in 1770, and his widow received £3000 from the English nation. At the same time, Euler, whose Tables had been the origin and foundation of Mayer's, also had a recompense of the same amount.

This public national acknowledgment of the practical accuracy of these Tables is, it will be observed, also a solemn recognition of the truth of the Newtonian theory, as far as truth can be judged of by men acting under the highest official responsibility, and aided by the most complete command of the resources of the skill and talents of others. The finding the longitude is thus the seal of the moon's gravitation to the sun and earth; and with this occurrence, therefore, our main concern with the history of the lunar theory ends. Various improvements have been since introduced into this research; but on these we, with so many other subjects before us, must forbear to enter.

Sect. 3.—*Application of the Newtonian Theory to the Planets, Satellites, and Earth.*

THE theories of the Planets and Satellites, as affected by the law of universal gravitation, and therefore by perturbations, were naturally subjects of interest, after the promulgation of that law. Some of the effects of the mutual attraction of the planets had, indeed, already attracted notice. The inequality produced by the mutual attraction of Jupiter and Saturn cannot be overlooked by a good observer. In the preface to the second edition of the Principia, Cotes remarks[17], that the perturbation of Jupiter and Saturn is not unknown to astronomers. In Halley's Tables it was noticed[18] that there are very great deviations from regularity in these two planets, and these deviations are ascribed to the perturbing force of the planets on each other; but the correction of these by a suitable equation is left to succeeding astronomers.

The motion of the planes and apsides of the planetary orbits was one of the first results of their mutual perturbation which was observed. In 1706, La Hire and Maraldi compared Jupiter with the Rudolphine Tables, and those of Bullialdus; it appeared that his aphelion had advanced, and that his nodes had regressed. In 1728, J. Cassini found that

[17] Pref. to Princip. p. xxi. [18] End of Planetary Tables.

Saturn's aphelion had in like manner travelled forwards. In 1720, when Louville refused to allow in his solar tables the motion of the aphelion of the earth, Fontenelle observed that this was a misplaced scrupulousness, since the aphelion of Mercury certainly advances. Yet this reluctance to admit change and irregularity was not yet overcome. When astronomers had found an approximate and apparent constancy and regularity, they were willing to believe it absolute and exact. In the satellites of Jupiter, for instance, they were unwilling to admit even the eccentricity of the orbits; and still more, the variation of the nodes, inclinations, and apsides. But all the fixedness of these was successively disproved. Fontenelle in 1732, on the occasion of Maraldi's discovery of the change of inclination of the fourth satellite, expresses a suspicion that all the elements might prove liable to change. "We see," says he, "the constancy of the inclination already shaken in the three first satellites, and the eccentricity in the fourth. The immobility of the nodes holds out so far, but there are strong indications that it will share the same fate."

The motions of the nodes and apsides of the satellites are a necessary part of the Newtonian theory; and even the Cartesian astronomers now required only data, in order to introduce these changes into their tables.

The complete reformation of the Tables of the Sun, Planets, and Satellites, which followed as a

natural consequence, from the revolution which Newton had introduced, was rendered possible by the labours of the great constellation of mathematicians of whom we have spoken in the last book, Clairaut, Euler, D'Alembert, and their successors; and it was carried into effect in the course of the last century. Thus Lalande applied Clairaut's theory to Mars, as did Mayer; and the inequalities in this case, says Bailly[19] in 1785, may amount to two minutes, and therefore must not be neglected. Lalande determined the inequalities of Venus, as did Father Walmesley, an English mathematician; these were found to reach only to thirty seconds.

The planetary tables[20] which were in highest repute, up to the end of the last century, were those of Lalande. In these, the perturbations of Jupiter and Saturn were introduced, their magnitude being such that they cannot be dispensed with; but the tables of Mercury, Venus, and Mars, had no perturbations. Hence these latter tables might be considered as accurate enough to enable the observer to find the object, but not to test the theory of perturbations. But when the calculation of the mutual disturbances of the planets was applied, it was always found that it enabled mathematicians to bring the theoretical places to coincide more exactly with those observed. In improving, as much as possible, this coincidence, it is necessary to determine the mass of

[19] Ast. Mod. iii. 170. [20] Airy. Report on Ast. to Brit. Ass. 1832.

each planet; for upon that, according to the law of universal gravitation, its disturbing power depends. Thus, in 1813, Lindenau published tables of Mercury, and concluded, from them, that a considerable increase of the supposed mass of Venus was necessary to reconcile theory with observation[21]. He had published tables of Venus in 1810, and of Mars in 1811. And, in proving Bouvard's tables of Jupiter and Saturn, values were obtained of the masses of those planets. The form in which the question of the truth of the doctrine of universal gravitation now offers itself to the minds of astronomers is this:—that it is taken for granted that it will account for the motions of the heavenly bodies, and the question is, with what supposed masses it will give the *best* account[22]. The continually-increasing accuracy of the tables shows the truth of the fundamental assumption.

The question of perturbation is exemplified in the satellites also. Thus the satellites of Jupiter are not only disturbed by the sun, as the moon is, but also by each other, as the planets are. This mutual

[21] Airy, Report on Ast. to Brit. Ass. 1832.

[22] Among the most important corrections of the supposed masses of the planets, we may notice that of Jupiter, by Professor Airy. This determination of Jupiter's mass was founded, not on the effect as seen in perturbations, but on a much more direct datum, the time of revolution of his fourth satellite. It appeared, from this calculation, that Jupiter's mass required to be increased by about 1-80th. This result has been generally adopted as an improvement of the elements of our system.

action gives rise to some very curious relations among their motions; which, like most of the other leading inequalities, were forced upon the notice of astronomers by observation before they were obtained by mathematical calculation. In Bradley's remarks upon his own tables of Jupiter's satellites, published among Halley's tables, he observes that the places of the three interior satellites are affected by errors which recur in a cycle of 437 days, answering to the time in which they return to the same relative position with regard to each other, and to the axis of Jupiter's shadow. Wargentin, who had noticed the same circumstance without knowledge of what Bradley had done, applied it, with all diligence, to the purpose of improving the tables of the satellites in 1746. But, at a later period, Laplace established, by mathematical reasoning, the very curious theorem on which this cycle depends, which he calls *the libration of Jupiter's satellites;* and Delambre was then able to publish tables of Jupiter's satellites more accurate than those of Wargentin, which he did in 1789[23].

The progress of physical astronomy from the time of Euler and Clairaut, has consisted in a series of calculations and comparisons of the most abstruse and recondite kind. The formation of tables of the planets and satellites from the theory required the solution of problems much more complex than the

[23] Voiron, Hist. Ast. p. 322.

original case of the problem of three bodies. The real motions of the planets and their orbits are rendered still further intricate by this, that all the lines and points to which we can refer them, are themselves in motion. The task of carrying order and law into this mass of apparent confusion, has required a long series of men of transcendent intellectual powers; and a perseverance and delicacy of observation, such as we have not the smallest example of in any other subject. It is impossible here to give any account of these labours; but we may mention one instance. The nodes of Jupiter's fourth satellite do not go backwards[24], as the Newtonian theory seems to require; they advance upon Jupiter's orbit. But then, it is to be recollected that the theory requires the nodes to retrograde upon the orbit of the perturbing body, which is here the third satellite; and Lalande showed that, by the necessary relations of space, the latter motion may be retrograde though the former is direct.

Attempts have been made, from the time of the solution of the problem of three bodies to the present, to give the greatest possible accuracy to the Tables of the Sun, by considering the effect of the various perturbations to which the earth is subject. Thus, in 1756, Euler calculated the effect of the attractions of the planets on the earth (the prize-question of the French Academy of Sciences) and

[24] Bailly, iii. 175.

Clairaut soon after. Lacaille, making use of these results, and of his own numerous observations, published tables of the sun. In 1786, Delambre[25] undertook to verify and improve these tables, by comparing them with 314 observations made by Maskelyne, at Greenwich, in 1775 and 1784, and in some of the intermediate years. He corrected most of the elements; but he could not remove the uncertainty which occurred respecting the amount of the inequality produced by the reaction of the moon. He admitted also, in pursuance of Clairaut's theory, a second term of this inequality depending on the moon's latitude; but irresolutely, and half-disposed to reject it on the authority of the observations. Succeeding researches of mathematicians have shown, that this term is not admissible as a result of mechanical principles. Delambre's tables, thus improved, were exact to seven or eight seconds[26]; which was thought, and truly, a very close coincidence for the time. But astronomers were far from resting content with this. In 1806, the French Board of Longitude published Delambre's improved solar tables; and in the Connaissance des Tems, for 1816, Burckhardt gave the results of a comparison of Delambre's tables with a great number of Maskelyne's observations;—far greater than the number on which they were founded[27]. It appeared that

[25] Voiron. Hist. p. 315. [26] Montucla. iv. 42.
[27] Airy. Report, p. 150.

the epoch, the perigee, and the eccentricity, required sensible alterations, and that the mass of Venus ought to be reduced about one-ninth, and that of the Moon to be sensibly diminished. In 1827, Professor Airy[88] of Cambridge, compared Delambre's tables with 2000 Greenwich observations, made with the new transit-instrument, and deduced from this comparison the correction of the elements. These in general agreed closely with Burckhardt's, excepting that a diminution of Mars appeared necessary. Some discordancies, however, led Professor Airy to suspect the existence of an inequality which had escaped the sagacity of Laplace and Burckhardt. And, a few weeks after this suspicion had been expressed, the same mathematician announced to the Royal Society that he had detected, in the planetary theory, such an inequality, hitherto unnoticed. Its whole effect on the earth's longitude, would be to increase or diminish it by nearly three seconds of space, and its period is about 240 years. "This term," he adds, "accounts completely for the difference of the secular motions given by the comparisons of the epochs of 1783 and 1821, and by that of the epochs of 1801 and 1821."

Many excellent tables of the motions of the sun, moon, and planets, were published in the latter part of last century; but the Bureau des Longitudes, which was established in France in 1795, endea-

[88] Phil. Trans. 1828.

voured to give new or improved tables of most of these motions. Thus were produced Delambre's tables of the sun, Burg's tables of the moon, Bouvard's tables of Jupiter, Saturn, and Uranus. The agreement between these and observation is, in general, truly marvellous. Yet astronomers still endeavour to improve this accordance. Thus in the preface to the tables of Uranus, Bouvard says in 1812[29], "the formation of these tables offers us this alternative, that we cannot satisfy the modern observations to the suitable degree of precision, without making our tables deviate from the ancient observations." This accordingly he has done; but the consequence is, that at present, in 1836, Uranus deviates four seconds of time from his calculated place, and thus indicates some defect still existing in the tables.

We may notice here a difference in the mode of referring to observation when a theory is first established, and when it is afterwards to be confirmed and corrected. It was remarked as a merit in the method of Hipparchus, and an evidence of the mathematical coherence of his theory, that in order to determine the place of the sun's apogee, and the eccentricity of his orbit, he required to know nothing besides the lengths of winter and spring. But if the fewness of the requisite data is a beauty in the first fixation of a theory, the multitude of observations to which it applies is its excellence when it is

[29] Pref. p. xiv.

established; and in correcting tables, mathematicians take far more data than would be requisite to determine the elements. For the theory ought to account for *all* the facts: and since it will not do this with mathematical rigour (for observation is not perfect), the elements are determined, not so as to satisfy any selected observations, but so as to make the whole mass of error as small as possible. And thus in the adaptation of theory to observation, even in its most advanced state, there is room for sagacity and skill, prudence and judgment.

In this manner, by selecting the best mean elements of the motions of the heavenly bodies, they deviate from this mean in the way the theory points out, and constantly return to it. To this general rule, of the constant return to a mean, there are, however, some apparent exceptions, of which we shall now speak.

Sect. 3.—*Application of the Newtonian Theory to Secular Inequalities.*

SECULAR inequalities in the motions of the heavenly bodies occur in consequence of changes in the elements of the solar system, which go on progressively from age to age. The example of such changes which was first studied by astronomers, was the acceleration of the moon's mean motion, discovered by Halley. The observed fact was, that the moon now moves in a very small de[illegible]ee quicker than she did in the earlier ages of

the world. When this was ascertained, the various hypotheses which appeared likely to account for the fact were reduced to calculation. The resistance of the medium in which the heavenly bodies move was the most obvious of these hypotheses. Another, which was for some time dwelt upon by Laplace, was the successive transmission of gravity, that is, the hypothesis that the gravity of the earth takes a certain finite time to reach the moon. But none of these suppositions gave satisfactory conclusions; and the strength of Euler, D'Alembert, Lagrange, and Laplace, was foiled by this difficulty. At length, in 1787, Laplace announced to the Academy that he had discovered the true cause of this acceleration, and that it arose from the action of the sun upon the moon, combined with the secular variation of the eccentricity of the earth's orbit. It was found that the effects of this combination would exactly account for the changes which had hitherto so perplexed mathematicians. A very remarkable result of this investigation was, that "this secular inequality of the motion of the moon is periodical, but it requires millions of years to re-establish itself;" so that after an almost inconceivable time, the acceleration will become a retardation. Laplace some time after (in 1797,) announced other discoveries relative to the secular motions of the apogee and the nodes of the moon's orbit. And in 1802, he pointed out a small inequality of the moon, of which the period is 154 years, and which, he then

thought, explained some discrepancies which Burg had found in constructing his lunar tables. Laplace collected these researches in his "Theory of the Moon," which he published in the third volume of the "Mécanique Céleste" in 1802.

A similar case occurred with regard to an acceleration of Jupiter's mean motion, and a retardation of Saturn's, which had been observed by Cassini, Maraldi, and Horrox. After several imperfect attempts by other mathematicians, Laplace in 1787 found that there resulted from the mutual attraction of those two planets a great inequality, of which the period is 929 years and a half, and which has accelerated Jupiter and retarded Saturn ever since the restoration of astronomy.

Thus the secular inequalities of the celestial motions, like all the others, confirm the law of universal gravitation. They are called secular, because ages are requisite to unfold their existence, and because they are not obviously periodical. They might, in some measure, be considered as extensions of the Newtonian theory, for though Newton's law accounts for such facts, he did not, so far as we know, foresee such a result of it. But on the other hand, they are exactly of the same nature as those which he did foresee and calculate. And when we call them *secular*, in opposition to *periodical*, it is not that there is any real difference, for they, too, have their cycle; but it is that we have assumed our mean motion without allowing for these long inequalities.

And thus, as Laplace observes on this very occasion[30], the lot of this great discovery of gravitation is no less than this, that every apparent exception becomes a proof, every difficulty a new occasion of a triumph. And such, as he truly adds, is the character of a true theory,—of a real representation of nature.

It is impossible for us here to enumerate even the principal objects which have thus filled the triumphal march of the Newtonian theory from its outset up to the present time. But among these secular changes, we may mention the diminution of the obliquity of the ecliptic, which has been going on from the earliest times to the present. This has been explained by theory, and shown to have, like all the other changes of the system, a limit, after which the diminution will be converted into an increase.

We may mention here some subjects of a kind somewhat different from those just spoken of. The true theoretical quantity of the precession of the equinoxes, which had been erroneously calculated by Newton, was shown by D'Alembert to agree with observation. The coincidence of the nodes of the moon's equator with those of her orbit, was proved to result from mechanical principles by Lagrange. The curious circumstance that the time of the moon's rotation on her axis is equal to the time of her revolution

[30] Syst. du Monde. 8vo. ii. 37.

about the earth, was shown to be consistent with the results of the laws of motion by Laplace. Laplace also, as we have seen, explained certain remarkable relations which constantly connect the longitudes of the three first satellites of Jupiter; Bailly and Lagrange analysed and explained the curious librations of the nodes and inclinations of their orbits; and Laplace traced the effect of Jupiter's oblate figure on their motions, which masks the other causes of inequality, by determining the direction of the motions of the *perijove* and node of each satellite.

Sect. 4.—*Application of the Newtonian Theory to the New Planets.*

We are now so accustomed to consider the Newtonian theory as true, that we can hardly imagine to ourselves the possibility that those planets which were not discovered when the theory was founded, should contradict its doctrines. We can scarcely conceive it possible that Uranus or Ceres should have been found to violate Kepler's laws, or to move without suffering perturbations from Jupiter and Saturn. Yet if we can suppose men to have had any doubt of the exact and universal truth of the doctrine of universal gravitation, at the period of these discoveries, they must have scrutinised the motions of these new bodies with an interest far more lively than that with which we now look for the predicted return of a comet. The solid esta-

blishment of the Newtonian theory is thus shown by the manner in which we take it for granted not only in our reasonings, but in our feelings. But though this is so, a short notice of the process by which the new planets were brought within the domain of the theory may properly find a place here.

William Herschel, a man of great energy and ingenuity, who had made material improvements in reflecting telescopes, observing at Bath on the 13th of March, 1781, discovered, in the constellation Gemini, a star larger and less luminous than the fixed stars. On the application of a more powerful telescope, it was seen magnified, and two days afterwards he perceived that it had changed its place. The attention of the astronomical world was directed to this new object, and the best astronomers in every part of Europe employed themselves in following it along the sky[31].

The admission of an eighth planet into the long established list, was a notion so foreign to men's thoughts at that time, that other suppositions were first tried. The orbit of the new body was at first calculated as if it had been a comet running in a parabolic path. But in a few days the star deviated from the course thus assigned it: and it was in vain that in order to represent the observations, the perihelion distance of the parabola was increased from fourteen to eighteen times the earth's distance

[31] Voiron. Hist. Ast. p. 12.

from the sun. Saron, of the Academy of Sciences of Paris, is said[32] to have been the first person who perceived that the places were better represented by a circle than by a parabola: and Lexell, a celebrated mathematician of Petersburg, found that a motion in a circular orbit, with a radius double of that of Saturn, would satisfy all the observations. This made its period about eighty-two years.

Lalande soon discovered that the circular motion was subject to a sensible inequality: the orbit was, in fact, an ellipse, like those of the other planets. To determine the equation of the centre of a body which revolves so slowly, would, according to the ancient methods, have required many years: but Laplace contrived methods by which the elliptical elements were determined from four observations, within little more than a year from its first discovery by Herschel. These calculations were soon followed by tables of the new planet, published by Nouet.

In order to obtain additional accuracy, it now became necessary to take account of the perturbations. The French Academy of Sciences proposed, in 1789, the construction of new tables of this planet as its prize-question. It is a curious illustration of the constantly accumulating evidence of the theory, that the calculation of the perturbations of the planet enabled astronomers to discover that it had been observed as a star in three different positions in

[32] Voiron. Hist. Ast. p. 12.

former times; namely, by Flamsteed in 1690, by Mayer in 1756, and by Le Monnier in 1769. Delambre, aided by this discovery and by the theory of Laplace, calculated tables of the planet, which, being compared with observation for three years, never deviated from it more than seven seconds. The Academy awarded its prize to these tables, they were adopted by the astronomers of Europe, and the planet of Herschel now conforms to the laws of attraction, no less than those ancient members of the known system from which the theory was inferred.

The history of the discovery of the other new planets, Ceres, Pallas, Juno, and Vesta, is nearly similar to that just related, except that their planetary character was more readily believed. The first of these was discovered on the first day of this century by Piazzi, the astronomer at Palermo; but he had only begun to suspect its nature, and had not completed his third observation, when his labours were suspended by a dangerous illness: and on his recovery the star was invisible, being lost in the rays of the sun.

He declared it to be a planet with an elliptical orbit; but the path which it followed, on emerging from the neighbourhood of the sun, was not that which Piazzi had traced out for it. Its extreme smallness made it difficult to rediscover; and the whole of the year 1801 was employed in searching the sky for it in vain. At last, after many trials,

Von Zach and Olbers again found it, the one on the last day of 1801, the other on the first day of 1802. Gauss and Burckhardt immediately used the new observations in determining the elements of the orbit; and the former invented a new method for the purpose. Ceres now moves in a path of which the course and inequalities are known, and can no more escape the scrutiny of astronomers.

The second year of the nineteenth century also produced its planet. This was discovered by Dr. Olbers, a physician of Bremen, while he was searching for Ceres among the stars of the constellation Virgo. He found a star which had a perceptible motion even in the space of two hours. It was soon announced as a new planet, and received from its discoverer the name of Pallas. As in the case of Ceres, Burckhardt and Gauss employed themselves in calculating its orbit. But some peculiar difficulties here occured. Its eccentricity is greater than that of any of the old planets, and the inclination of its orbit to the ecliptic is not less than thirty-five degrees. These circumstances both made its perturbations large, and rendered them difficult to calculate. Burckhardt employed the known processes of analysis, but they were found insufficient: and the Imperial Institute (as the French Academy was termed during the reign of Napoleon,) proposed the perturbations of Pallas as a prize-question.

To these discoveries succeeded others of the same kind. The German astronomers agreed to examine

the whole of the zone in which Ceres and Pallas move; in the hope of finding other planets, fragments, as Olbers conceived they might possibly be, of one original mass. In the course of this research, Mr. Harding of Lilienthal, on the 1st of September, 1804, found a new star, which he soon was led to consider as a planet. Gauss and Burckhardt also calculated the elements of this orbit, and the planet was named Juno.

After this discovery, Olbers sought the sky for additional fragments of his planet with extraordinary perseverance. He conceived that one of two opposite constellations, the Virgin or the Whale, was the place where its separation must have taken place; and where, therefore, all the orbits of all the portions must pass. He resolved to survey, three times a year, all the small stars in these two regions. This undertaking, so curious in its nature, was successful. The 29th of March, 1807, he discovered Vesta, which was soon found to be a planet. And to show the manner in which Olbers pursued his labours, we may state that he afterwards published a notification that he had examined the same parts of the heavens with such regularity, that he was certain no new planet had passed between 1808 and 1816. Gauss and Burckhardt computed the orbit of Vesta; and when Gauss compared one of his orbits with twenty-two observations of M. Bouvard, he found the errors below seventeen seconds of space in right ascension, and still less in declination.

The elements of all these orbits have been successively improved, and this has been done entirely by the German mathematicians[33]. These perturbations are calculated, and the places for some time before and after opposition are now given in the Berlin Ephemeris. "I have lately observed," says Professor Airy, "and compared with the Berlin Ephemeris, the right ascensions of Juno and Vesta, and I find that they are rather more accurate than those of Venus:" so complete is the confirmation of the theory by these new bodies; so exact are the methods of tracing the theory to its consequences.

We may observe that all these new-discovered bodies have received names taken from the ancient mythology. In the case of the first of these, astronomers were originally divided; the discoverer himself named it the *Georgium Sidus*, in honour of his patron, George the Third; Lalande and others called it *Herschel*. Nothing can be more just than this mode of perpetuating the fame of the author of a discovery; but it was felt to be ungraceful to violate the homogeneity of the ancient system of names. Astronomers tried to find for the hitherto neglected denizen of the skies, an appropriate place among the deities to whose assembly he was at last admitted; and *Uranus*, the father of Saturn, was fixed upon as best suiting the order of the course.

The mythological nomenclature of planets ap-

[33] Airy, Rep. 157.

peared from this time to be generally agreed to. Piazzi termed his, *Ceres Ferdinandea*. The first term, which contains a happy allusion to Sicily, the country of the discovery in modern, and of the goddess in ancient, times, has been accepted; the attempt to pay a compliment to royalty out of the products of science, in this as in most other cases, has been set aside. Pallas, Juno, and Vesta, were named, without any peculiar propriety of selection, according to the choice of their discoverers.

Sect. 5.—*Application of the Newtonian Theory to Comets.*

A FEW words must be said upon another class of bodies, which at first seemed as lawless as the clouds and winds; and which astronomy has reduced to a regularity as complete as that of the sun;—upon *Comets*. No part of the Newtonian discoveries excited a more intense interest than this. These anomalous visitants were anciently gazed at with wonder and alarm; and might still, as in former times, be accused of "perplexing nations," though with very different fears and questionings. The conjecture that they, too, obeyed the law of universal gravitation, was to be verified by showing that they described a curve such as that force would produce. Hevelius, who was a most diligent observer of these objects, had, without reference to gravitation, satisfied himself that they

moved in parabolas[34]. To determine the elements of the parabola from observations, even Newton called[35] "problema longe difficillimum." Newton determined the orbit of the comet of 1680 by certain graphical methods. His methods supposed the orbit to be a parabola, and satisfactorily represented the motion in the visible part of the comet's path. But this method did not apply to the possible return of the wandering star. Halley has the glory of having first detected a periodical comet, in the case of that which has since borne his name. But this great discovery was not made without labour. In 1705, Halley[36] explained how the parabolic orbit of a planet may be determined from three observations; and, joining example to precept, himself calculated the positions and orbits of twenty-four comets. He found, as the reward of this industry, that the comets of 1607, and of 1531, had the same orbit as that of 1682. And here the intervals are also nearly the same, namely, about seventy-five years. Are the three comets then identical? In looking back into the history of such appearances, he found comets recorded in 1456, in 1380, and in 1305; the intervals are still the same, seventy-five or seventy-six years. It was impossible now to doubt that they were the periods of a revolving body; that the comet was a planet; its orbit a long ellipse, not a parabola.

[34] Bailly, ii. 246. [35] Principia, ed. i. p. 494.
[36] Bailly, ii. 646.

But if this were so, the comet must reappear in 1758 or 1759. Halley predicted that it would do so; and the fulfilment of this prediction was naturally looked forwards to, as an additional stamp of the truths of the theory of gravitation.

But in all this, the comet had been supposed to be affected only by the attraction of the sun. The planets must disturb its motion as they disturb each other. How would this disturbance affect the time and circumstances of its reappearance? Halley had proposed, but not attempted to solve, this question.

The effect of perturbations upon a comet defeats all known methods of approximation, and requires immense labour. "Clairaut," says Bailly[37], "undertook this: with courage enough to dare the adventure, he had talent enough to obtain a memorable victory:" the difficulties, the labours, grew upon him as he advanced, but he fought his way through them, assisted by Lalande, and by a female calculator, Madame Lepaute. He predicted that the comet would reach its perihelion April 13, 1759, but claimed the license of a month for the inevitable errors of a calculation which, in addition to all other sources of error, was made in haste, that it might appear as a prediction. The comet justified his calculations and his caution together; for it arrived at its perihelion on the 13th of March.

Two other comets, of much shorter period, have

[37] Bailly, A. M. iii. 190.

been detected of late years; Encke's, which revolves round the sun in three years and one third, and Gambart's, which describes an ellipse, not extremely eccentric, in six years and three quarters. These bodies, apparently thin and vaporous masses, like other comets, have, since their orbits were calculated, punctually conformed to the law of gravitation. If it were still doubtful whether the more conspicuous comets do so, these bodies would tend to prove the fact, by showing it to be true in an intermediate case.

We may add to the history of comets, that of Lexell's, which, in 1770, appeared to be revolving in a period of about five years, and whose motion was predicted accordingly. The prediction was disappointed; but the failure was sufficiently explained by the comet's having passed close to Jupiter, by which occurrence its orbit was utterly deranged.

Thus, no verification of the Newtonian theory, which was possible in the motions of the stars, has yet been wanting. The return of Halley's comet, in 1835, and the extreme exactitude with which it conformed to its predicted course, is a testimony of truth, which must appear striking even to the most incurious respecting such matters.

We have spoken of three comets which, unlike the planets, take their names from those mathematicians who proved them to be bodies that revolve round the sun repeatedly;—those of Halley, Encke, and Gambart. The latter is sometimes called Biela's

comet, from the name of a German officer, who first *saw* it: but the proof of its revolution round the sun is a step so much more important, that we cannot but assent to the justice of what is urged by M. Arago[38], that this ought to be established as the ground of nomenclature in such cases, and that the comet ought to bear the name of the French astronomer, Gambart.

Sect. 6.—*Application of the Newtonian Theory to the Figure of the Earth.*

THE heavens had thus been consulted respecting the Newtonian doctrine, and the answer given, over and over again, in a thousand different forms, had been, that it was true; nor had the most persevering cross-examination been able to establish anything of contradiction or prevarication. The same question was also to be put to the earth and the ocean, and we must briefly notice the result.

According to the Newtonian principles, the form of the earth must be a globe somewhat flattened at the poles. This conclusion, or at least the amount of the flattening, depends not only upon the existence and law of attraction, but upon its belonging to each particle of the mass separately; and thus the experimental confirmation of the form asserted from calculation, would be a verification of the theory in

[38] Eloge de Gambart.

its widest sense. The application of such a test was the more necessary to the interests of science, inasmuch as the French astronomers had collected from their measures, and had connected with their Cartesian system, the opinion that the earth was not oblate but oblong. Dominic Cassini had measured seven degrees of latitude from Amiens to Perpignan, in 1701, and found them to decrease in going from south to north. The prolongation of this measure to Dunkirk confirmed the same result. But if the Newtonian doctrine was true, the contrary ought to be the case, and the degrees ought to increase in proceeding towards the pole.

The only answer which the Newtonians could at this time make to the difficulty thus presented, was, that an arc so short as that thus measured, was not to be depended upon for the determination of such a question; inasmuch as the inevitable errors of observation might exceed the differences which were the object of research. It would, undoubtedly, have become the English to have given a more complete answer, by executing measurements under circumstances not liable to this uncertainty. The glory of doing this, however, they, for a long time, abandoned to other nations. The French undertook the task with great spirit[39]. In 1733, in one of the meetings of the French Academy, when this question was discussed, De la Condamine, an ardent and eager

[39] Bailly, iii. 11.

man, proposed to settle this question by sending members of the academy to measure a degree of the meridian at the equator, in order to compare it with the French degrees, and offered himself for the expedition. Maupertuis, in like manner, urged the necessity of another expedition to measure a degree in the neighbourhood of the pole. The government received the applications favourably, and these remarkable scientific missions were sent out at the national expense.

From this time there was no longer any doubt as to the fact of the earth's oblateness, and the question only turned upon its quantity. Even before the return of the academicians, the Cassinis and La Caille had remeasured the French arc, and found errors which subverted the former result, making the earth oblate to the amount of 1-168th of its diameter. The expeditions to Peru and to Lapland had to struggle with difficulties in the execution of their design, which make their narratives resemble some romantic history of irregular warfare, rather than the monotonous records of mere measurements. The equatorial degree employed the observers not less than eight years. When they did return, and their results were compared, their discrepancy, as to quantity, was considerable. The comparison of the Peruvian and French arcs gave an ellipticity of nearly 1-314th, that of the Peruvian and Swedish arcs gave 1-213th for its value.

Newton had deduced from his theory, by reason-

ings of singular ingenuity, an ellipticity of 1-230th; but this result had been obtained by supposing the earth homogeneous. If the earth be, as we should most readily conjecture it to be, more dense in its interior than at its exterior, the ellipticity will be less than that of a homogeneous spheroid revolving in the same time. It does not appear that Newton was aware of this; but Clairaut, in 1743, in his "Figure of the Earth," proved this and many other important results of the attraction of the particles. Especially he established that, in proportion as the fraction expressing the ellipticity becomes smaller, that expressing the excess of the polar over the equatorial gravity becomes larger; and he thus connected the measures of the ellipticity obtained by means of degrees, with those obtained by means of pendulums in different latitudes.

The altered rate of a pendulum, when carried towards the equator, had been long ago observed by Richer and Halley, and had been quoted by Newton as confirmatory of his theory. Pendulums were swung by the academicians who measured the degrees, and confirmed the general character of the results.

But having reached this point of the verification of the Newtonian theory, any additional step becomes more difficult. Many excellent measures, both of degrees and of pendulums, have been made since those just mentioned. The results of the arcs[40] is

[40] Airy. Fig. Earth, p. 230.

an ellipticity of 1-298th;—of the pendulums, an ellipticity of about 1-285th. This difference is considerable, if compared with the quantities themselves; but does not throw a shadow of doubt on the truth of the theory. Indeed, the observations of each kind exhibit irregularities which we may easily account for, by ascribing them to the unknown distribution of the denser portions of the earth, but which preclude the extreme of accuracy and certainty in our result.

But the near agreement of the determination, from degrees and from pendulums, is not the only coincidence by which the doctrine is confirmed. We can trace the effect of the earth's oblateness in certain minute apparent motions of the stars; for the attraction of the sun and moon on the protuberant matter of the spheroid produces the precession of the equinoxes, and a nutation of the earth's axis. The precession had been known from the time of Hipparchus, and the existence of nutation was foreseen by Newton; but the quantity is so small, that it required consummate skill and great labour in Bradley to detect it by astronomical observation. Being, however, so detected, its amount, as well as that of the precession, gives us the means of determining the amount of terrestrial ellipticity, by which the effect is produced. But it is found, upon calculation, that we cannot obtain this determination without assuming some law of density in the homogeneous strata of which we suppose the earth

to consist[41]. The density will certainly increase in proceeding towards the centre, and there is a simple and probable law of this increase, which will give 1-300th for the ellipticity, and from the amount of two lunar inequalities, (one in latitude and one in longitude,) which are produced by the earth's oblateness. Nearly the same result follows from the quantity of nutation. Thus everything tends to convince us that the ellipticity cannot deviate much from this fraction.

Sect. 7.—Confirmation of the Newtonian Theory by Experiments on Attraction.

THE attraction of all the parts of the earth to one another was thus proved by experiments, in which the whole mass of the earth is concerned. But attempts have also been made to measure the attraction of smaller portions; as mountains, or artificial masses. This is an experiment of great difficulty; for the attraction of such masses must be compared with that of the earth, of which it is a scarcely-perceptible fraction; and, moreover, in the case of mountains, the effect of the mountain will be modified or disguised by unknown or unappreciable circumstances. In many of the measurements of degrees, indications of the attraction of mountains had been perceived; but at the suggestion of Mas-

[41] Airy. Fig. Earth, p. 235.

kelyne, the experiment was carefully made, in 1774, upon the mountain Schehallien, in Scotland. The result obtained was, that the attraction of the mountain drew the plumb-line about six seconds from the vertical; and it was deduced from this, by Hutton's calculations, that the density of the earth was about once and four-fifths that of Schehallien.

Cavendish, who had suggested many of the artifices in this calculation, himself made the experiment in the other form, by using leaden balls, about nine inches diameter. This observation was conducted with an extreme degree of ingenuity and delicacy, which could alone make it valuable; and the result agreed very nearly with that of the Schehallien experiment, giving for the density of the earth about five and one-third times that of water. Nearly the same result was obtained by Carlini, in 1824, from observations of the pendulum, made at a point of the Alps (the Hospice, on Mount Cenis) at a considerable elevation above the average surface of the earth.

Sect. 8.—*Application of the Newtonian Theory to the Tides.*

We come, finally, to that result, in which most remains to be done for the verification of the general law of attraction; the subject of the Tides. Yet, even here, the verification is striking as far as observations have been carried. Newton's theory explained, with singular felicity, all the prominent circumstances

of the tides then known; the difference of spring and neap tides; the effect of the moon's and sun's declination and parallax; even the difference of morning and evening tides, and the anomalous tides of particular places. About, and after, this time, attempts were made both by the Royal Society of England, and by the French Academy, to collect numerous observations; but these were not followed up with sufficient perseverance. Perhaps, indeed, the theory had not been at that time sufficiently developed; but the admirable prize-essays of Euler, Bernoulli, and D'Alembert, in 1740, removed, in a great measure, this deficiency. These dissertations supplied the means of bringing this subject to the same test to which all the other consequences of gravitation had been subjected:—namely, the calculation of tables, and the continued and orderly comparison of these with observation. Laplace has attempted this verification in another way, by calculating the results of the theory (which he has done with an extraordinary command of analysis,) and then by comparing these, in supposed critical cases, with the Brest observations. This method has confirmed the theory as far as it could do so; but such a process cannot supersede the necessity of applying the proper criterion of truth in such cases, the construction and verification of tables. Bernoulli's theory, on the other hand, has been used for the construction of Tide-tables; but these have not been properly compared with experiment; and when

the comparison has been made, having been executed for purposes of gain rather than of science, it has not been published, and cannot be quoted as a verification of the theory.

Thus we have, as yet, no sufficient comparison of fact with theory, for Laplace's is far from a complete comparison. In this, as in other parts of physical astronomy, our theory ought not only to agree with observations selected and grouped in a particular manner, but with the whole course of observation, and with every part of the phenomena. In this, as in other cases, the true theory should be verified by its giving us the best tables; but tide-tables were never, I believe, calculated upon Laplace's theory, and thus it was never fairly brought to the test.

It is, perhaps, remarkable, considering all the experience which astronomy had furnished, that men should have expected to reach the completion of this branch of science by improving the mathematical theory, without, at the same time, ascertaining the laws of the facts. In all other departments of astronomy, as, for instance, in the cases of the moon and the planets, the leading features of the phenomena had been made out empirically, before the theory explained them. The course which analogy would have recommended for the cultivation of our knowledge of the tides, would have been, to ascertain, by an analysis of long series of observations, the effect of changes in the time of transit, parallax, and declination of the moon, and thus to obtain the

laws of phenomena; and then to proceed to investigate the laws of causation.

Though this was not the course followed by mathematical theorists, it was really pursued by those who practically calculated tide-tables; and the application of knowledge to the useful purposes of life being thus separated from the promotion of the theory, was naturally treated as a gainful property, and preserved by secrecy. Art, in this instance, having cast off her legitimate subordination to science, or rather, being deprived of the guidance which it was the duty of science to afford, resumed her ancient practices of exclusiveness and mystery. Liverpool, London, and other places, had their tide-tables, constructed by undivulged methods, which methods, in some instances at least, were handed down from father to son for several generations as a family-possession; and the publication of new tables accompanied by a statement of the mode of calculation, was resented as an infringement of the rights of property.

The mode in which these secret methods were invented, was that which we have pointed out;—the analysis of a considerable series of observations. Probably the best example of this was afforded by the Liverpool tide-tables. These were deduced by a clergyman named Holden, from observations made at that port by a harbour-master of the name of Hutchinson; who was led, by a love of such pursuits, to observe the tides carefully for above twenty years, day

and night. Holden's tables, founded on four years of these observations, were remarkably accurate.

At length men of science began to perceive that such calculations were part of their business; and that they were called upon, as the guardians of the established theory of the universe, to compare it in the greatest possible detail with the facts. Mr. Lubbock was the first mathematician who undertook the extensive labours which such a conviction suggested. Finding that regular tide-observations had been made at the London Docks from 1795, he took nineteen years of these (purposely selecting the length of a cycle of the motions of the lunar orbit,) and caused them (in 1831) to be analysed by Mr. Dessiou, an expert calculator. He thus obtained[48] tables for the effect of the moon's declination, parallax, and hour of transit, on the tides; and was enabled to produce tide-tables founded upon the data thus obtained. Some mistakes in these as first published (mistakes unimportant as to the theoretical value of the work,) served to show the jealousy of the practical tide-table calculators, by the acrimony with which the oversights were dwelt upon; but in a very few years, the tables thus produced by an open and scientific process, were more exact than those which resulted from any of the secrets; and thus practice was brought into its proper subordination to theory.

[48] Phil. Trans. 1831. British Almanac, 1832.

The theory with which Mr. Lubbock was led to compare his results, was the equilibrium-theory of Daniel Bernoulli; and it was found that this theory, with certain modifications of its elements, represented the facts to a remarkable degree of precision. Mr. Lubbock pointed out this feature especially in the semi-menstrual inequality of the times of high water. It was afterwards (in 1833) shown by Mr. Whewell[43] to obtain still more accurately at Liverpool, both for the tides and heights; for by this time, nineteen years of Hutchinson's Liverpool observations had also been discussed by Mr. Lubbock. The other inequalities of the times and heights (depending upon the declination and parallax of the moon and sun,) were variously compared with the equilibrium theory by Mr. Lubbock and Mr. Whewell; and the general result was, that the facts agreed with the condition of equilibrium at a certain anterior time, but that this anterior time was different for different phenomena. In like manner it appeared to follow from these researches, that in order to explain the facts, the mass of the moon must be supposed different in the calculation at different places; a result in effect the same was obtained by M. Daussy[44], an active French hydrographer; for he found that observations at various stations could not be reconciled with the formulæ of Laplace's Mécanique Céleste (in which the ratio of

[43] Phil. Trans. 1834. [44] Connaissance des Tems, 1838.

the heights of spring-tides and neap-tides was computed on an assumed mass of the moon) without an alteration of level which was, in fact, equivalent to an alteration of the moon's mass. Thus all things appeared to tend to show that the equilibrium-theory would give the formulæ for the inequalities of the tides, but that the magnitudes which enter into these formulæ must be sought from observation.

Whether this result is consistent with theory, is a question not so much of physical astronomy as of hydrodynamics, and has not yet been solved. A theory of the tides which should include in its conditions the phenomena of derivative tides, and of their combinations, will probably require all the resources of the mathematical mechanician.

As a contribution of empirical materials to the treatment of this hydrodynamical problem, it may be allowable to mention here Mr. Whewell's attempts to trace the progress of the tide into all the seas of the globe, by tracing what he calls *Cotidal Lines*;—lines marking the contemporaneous position of the various points of the great wave which carries high water from shore to shore[45]. This is necessarily a task of labour and difficulty, since it requires us to know the time of high water on the same day in every part of the world; but in proportion as it is completed, it supplies steps between our

[45] Essays towards an Approximation to a Map of Cotidal Lines. Phil. Trans. 1833, 1836.

general view of the movements of the ocean and the phenomena of particular ports.

Looking at this subject by the light which the example of the history of astronomy affords, we may venture to repeat, that it will never have justice done it till it is treated as other parts of astronomy are treated; that is, till tables of all the phenomena which can be observed, are calculated by means of the best knowledge which we at present possess, and till these tables are constantly improved by a comparison of the predicted with the observed fact. A set of tide-observations and tide-ephemerides of this kind, would soon give to this subject that precision which marks the other parts of astronomy; and would leave an assemblage of unexplained residual phenomena, in which a careful research might find the materials of other truths as yet unsuspected.

CHAPTER V.

DISCOVERIES ADDED TO THE NEWTONIAN THEORY.

Sect. 1.—*Tables of Astronomical Refraction.*

WE have travelled over an immense field of astronomical and mathematical labour in the last few pages, and have yet, at the end of every step, still found ourselves under the jurisdiction of the Newtonian laws. We are reminded of the universal monarchies, where a man could not escape from the empire without quitting the world. We have now to notice some other discoveries, in which this reference to the law of universal gravitation is less immediate and obvious; I mean the astronomical discoveries respecting light. The general truths to which the establishment of the true laws of atmospheric refraction led astronomers, were the law of deflection, which applies to all refractions, and the real structure and size of the atmosphere, so far as it is yet known. The great discoveries of Römer and Bradley, namely, the velocity of light, aberration, and nutation, gave to the former conceptions of the propagation of light a new distinctness, and confirmed the doctrines of Copernicus, Kepler, and

Newton, respecting the motions which belong to the earth.

The true laws of atmospheric refraction were slowly discovered. Tycho attributed the effect to the low and gross part of the atmosphere only, and hence made it cease at a point half-way to the zenith; but Kepler rightly extended it to the zenith itself. D. Cassini endeavoured to discover the law of this correction by observation, and gave his results in the form which, as we have said, sound science prescribes, a table to be habitually used for all observations. But great difficulties at this time embarrassed this investigation, for the parallaxes of the sun and of the planets were unknown, and very diverse values had been assigned them by different astronomers. To remove some of these difficulties, Richer, in 1762, went to observe at the equator; and on his return, Cassini was able to confirm and amend his former estimations of parallax and refraction. But there were still difficulties. According to La Hire, though the phenomena of twilight give an altitude of 34,000 toises to the atmosphere[1], those of refraction make it only 2000. John Cassini undertook to support and improve the calculations of his father Dominic, and took the true supposition, that the light follows a curvilinear path through the air. The Royal Society of London had already ascertained experimentally the refractive power of

[1] Bailly, ii. 612.

air[2]. Newton calculated a table of refractions, which was published under Halley's name in the "Philosophical Transactions for 1721," without any indication of the method by which it was constructed. But M. Biot has recently shown[3], by means of the published correspondence of Flamsteed, that Newton had solved the problem in a manner nearly corresponding to the most improved methods of modern analysis.

D. Cassini and Picard proved[4], Le Monnier in 1738 confirmed more fully, that the variations of the thermometer affect the refraction. Mayer, taking into account both these changes, and the changes indicated by the barometer, formed a theory, which La Caille, with immense labour, applied to the construction of a table of refractions from observation. But Bradley's Table (published in 1763 by Maskelyne,) was more commonly adopted in England; and his formula, originally obtained empirically, has been shown by Young to result from the most probable suppositions we can make respecting the atmosphere. Bessel's Refraction Tables are now considered the best of those which have appeared.

[2] Bailly, ii. 607.
[3] Biot, Acad. Sc. Compte Rendu. Sept. 5, 1836.
[4] Bailly, iii. 92.

Sect. 2.—Discovery of the Velocity of Light.—Römer.

THE astronomical history of refraction is not marked by any great discoveries, and was, for the most part, a work of labour only. The progress of the other portions of our knowledge is more striking. In 1676, a great number of observations of eclipses of Jupiter's satellites were accumulated, and could be compared with Cassini's Tables. Römer, a Danish astronomer, whom Picard had brought to Paris, perceived that these eclipses happened constantly later than the calculated time at one season of the year, and earlier at another season;—a difference for which astronomy could offer no account. The error was the same for all the satellites; if it had depended on a defect in the tables of Jupiter, it might have affected all, but the effect would have had a reference to the velocities of the satellites. The cause, then, was something extraneous to Jupiter. Römer had the happy thought of comparing the error with the earth's distance from Jupiter, and it was found that the eclipses happened later in proportion as Jupiter was further off. Thus we see the eclipse later, as it is more remote; and thus light, the messenger which brings us intelligence of the occurrence, travels over its course in a measurable time. By this evidence,

[s] Bailly, ii. 17.

light appeared to take about eleven minutes in describing the diameter of the earth's orbit.

This discovery, like so many others, once made, appears easy and inevitable; yet Dominic Cassini had entertained the idea for a moment, and had rejected it[6]; and Fontenelle had congratulated himself publicly on having narrowly escaped this seductive error. The objections to its admission arose principally from the inaccuracy of observation, and from the persuasion that the motions of the satellites were circular and uniform. Their irregularities disguised the fact in question. As these irregularities became clearly known, Römer's discovery was finally established, and the "equation of light" took its place in the tables.

Sect. 3.—*Discovery of Aberration. Bradley.*

IMPROVEMENTS in instruments, and in the art of observing, were requisite for making the next great step in tracing the effect of the laws of light. It appears clear, on consideration, that since light and the spectator on the earth are both in motion, the apparent direction of an object will be determined by the composition of these motions. But yet the effect of this composition was (as is usual in such cases) traced as a fact in observation, before it was seen as a consequence of reasoning. This fact,

[6] Bailly, ii. 419.

the aberration of light, the greatest astronomical discovery of the eighteenth century, belongs to Bradley, who was then Professor of Astronomy at Oxford, and afterwards Astronomer Royal at Greenwich. Molyneux and Bradley, in 1725, began a series of observations for the purpose of ascertaining, by observations near the zenith, the existence of an annual parallax of the fixed stars, which Hooke had hoped to detect, and Flamsteed thought he had discovered. Bradley[7] soon found that the star observed by him had a minute apparent motion different from that which the annual parallax would produce. He thought of a nutation of the earth's axis as a mode of accounting for this; but found, by comparison of a star on the other side of the pole, that this explanation would not apply. Bradley and Molyneux then considered for a moment an annual alteration of figure in the earth's atmosphere, such as might affect the refractions, but this hypothesis was soon rejected[8]. In 1727, Bradley resumed his observations, with a new instrument, at Wanstead; and obtained empirical rules for the changes of declination in different stars. At last, accident turned his thoughts to the direction in which he was to find the cause of the variations which he had discovered. Being in a boat on the Thames, he observed that the vane on the top of the mast gave a different apparent direction to the wind, as the boat sailed one

[7] Rigaud's Bradley. [8] Rigaud, p. xxiii.

way or the other. Here was an image of his case: the boat was the earth moving in different directions at different seasons, and the wind was the light of a star. He had now to trace the consequences of this idea; he found that it led to his empirical rules, and, in 1729, he gave his discovery to the Royal Society. His paper is a very happy narrative of his labours and his thoughts. His theory was so sound that no astronomer ever contested it; and his observations were so accurate, that the quantity which he assigned as the greatest amount of the change (one ninetieth of a degree) has hardly been corrected by more recent astronomers. It must be noticed, however, that he considered the effects in declination only; the effects in right ascension required a different mode of observation, and a consummate goodness in the machinery of clocks, which at that time was hardly attained.

Sect. 4.—*Discovery of Nutation.*

WHEN Bradley went to Greenwich as Astronomer Royal, he continued with perseverance observations of the same kind as those by which he had detected aberration. The result of these was another discovery; namely, that very nutation which he had formerly rejected. This may appear strange, but it is easily explained. The aberration is an annual change, and is detected by observing a star at different seasons of the year; the nutation is a change

of which the cycle is eighteen years; and which, therefore, though it does not much change the place of a star in one year, is discoverable in the alterations of several successive years. A very few years' observations showed Bradley the effect of this change[9]; and long before the half cycle of nine years had elapsed, he had connected it in his mind with the true cause, the motion of the moon's nodes. Machin was then secretary to the Royal Society[10], and was "employed in considering the theory of gravity, and its consequences with regard to the celestial motions:" to him Bradley communicated his conjectures; from him he soon received a table containing the results of his calculations; and the law was found to be the same in the table and in observation, though the quantities were somewhat different. It appeared by both, that the earth's pole, besides the motion which the precession of the equinoxes gives it, moves, in eighteen years, through a small circle;—or rather, as was afterwards found by Bradley, an ellipse, of which the axes are nineteen and fourteen seconds[11].

For the rigorous establishment of the mechanical theory of that effect of the moon's attraction from which the phenomena of nutation flow, Bradley rightly and prudently invited the assistance of the great mathematicians of his time. D'Alembert, Thomas Simpson, Euler, and others, answered this

[9] Rigaud, lxiv.

[10] Ib. 25.

[11] Ib. lxvi.

call, and the result was, as we have already said in the last chapter, that this investigation added another to the recondite and profound evidences of the doctrine of universal gravitation.

It has been said[18] that Bradley's discoveries "assure him the most distinguished place among astronomers after Hipparchus and Kepler." If his discoveries had been made before Newton's, there could have been no hesitation as to placing him on a level with those great men. The existence of such suggestions as the Newtonian theory offered on all astronomical subjects, may perhaps dim, in our eyes, the brilliance of Bradley's achievements; but this circumstance cannot place any other person above the author of such discoveries, and therefore we may consider Delambre's adjudication of precedence as well warranted, and deserving to be permanent.

Sect. 5.—*Discovery of the Laws of Double Stars. The two Herschels.*

No truth, then, can be more certainly established, than that the law of gravitation prevails to the very boundaries of the solar system. But does it hold good further? Do the fixed stars also obey this universal sway? The idea, the question, is an obvious one,—but where are we to find the means of submitting it to the test of observation?

[18] Delambre, Ast. du 18 Siéc. p. 420. Rigaud, xxxvii.

If the stars were each insulated from the rest, as our sun appears to be from them, we should have been quite unable to answer this inquiry. But among the stars, there are some which are called double, and which consist of two stars, so near to each other that the telescope alone can separate them. The elder Herschel diligently observed and measured such stars; and as has so often happened in astronomical history, pursuing one object he fell in with another. Supposing such pairs to be really unconnected, he wished to learn, from their phenomena, something respecting the annual parallax of the earth's orbit. But in the course of twenty years' observations he made the discovery (in 1803) that these couples were turning round each other with various angular velocities. These revolutions were, for the most part, so slow that he was obliged to leave their complete determination as an inheritance to the next generation. His son was not careless of the bequest, and after having added an enormous mass of observations to those of his father, he applied himself to determine the laws of these revolutions. A problem so obvious and so tempting was attacked also by others, as Savary and Encke, in 1830 and 1832, with the resources of analysis. But a problem in which the data are so minute and inevitably imperfect, required the mathematician to employ much judgment, as well as skill in using and combining these data; and Herschel, by employing positions only of the line joining the pair of stars,

to the exclusion of their distances, and by inventing a method which introduced the whole body of observations, and not selected ones only, into the determination of the motion, has made his investigations by far the most satisfactory of those which have appeared. The result is, that it has been rendered very probable, that the double stars describe ellipses about each other; and therefore that here also, at an immeasureable distance from our system, the law of attraction according to the inverse square, prevails. And, according to the practice of astronomers when a law has been established, tables have been calculated for the future motions; and we have ephemerides of the revolutions of suns round each other, in a region so remote, that the whole circle of our earth's orbit, if placed there, would be imperceptible by our strongest telescopes. The permanent comparison of the observed with the predicted motions, continued for more than one revolution, is the severe and decisive test of the truth of the theory: and the result of this test astronomers are now awaiting.

The verification of Newton's discoveries was sufficient employment for the last century; the first step in the extension of them belongs to this century. We cannot at present foresee the magnitude of this task, but every one must feel that the law of gravitation, before verified in all the particles of our own system, and now extended to the all but infinite distance of the fixed stars, presses upon our minds with

irresistible evidence, as a universal law of the whole material creation.

Thus, in this and the preceding chapter, I have given a brief sketch of the history of the verification and extension of Newton's great discovery. By the mass of labour and of skill which this head of our subject includes, we may judge of the magnitude of the advance in our knowledge which that discovery made. A wonderful amount of talent and industry have been requisite for this purpose; but with these, external means have co-operated. Wealth, authority, mechanical skill, the division of labour, the power of associations and of governments, have been largely and worthily applied in bringing astronomy to its present high and flourishing condition. We must consider briefly what has thus been done.

CHAPTER VI.

The Instruments and Aids of Astronomy during the Newtonian Period.

Sect. 1.—*Instruments.*

1. Some instruments or other were employed at all periods of astronomical observation. But it was only when observation had attained a considerable degree of delicacy, that the exact construction of instruments became an object of serious care. Gradually, as the possibility and the value of increased exactness became manifest, it was seen that everything which could improve the astronomer's instruments was of high importance to him. And hence in some cases a vast increase of size and of expense was introduced; in other cases new combinations, or the result of improvements in other sciences, were brought into play. Extensive knowledge, intense thought, and great ingenuity, were requisite in the astronomical instrument maker. Instead of ranking with artisans, he became a man of science, sharing the honour and dignity of the astronomer himself.

1. *Measure of Angles.*—Tycho Brahe was the first astronomer who acted upon a due appreciation of the importance of good instruments. The collection

of such at Uranibourg was by far the finest which had ever existed. He endeavoured to give steadiness to the frame, and accuracy to the divisions of his instruments. His mural quadrant was well adapted for this purpose; its radius was five cubits: it is clear, that as we enlarge the instrument we are enabled to measure smaller arcs. On this principle many large gnomons were erected. Cassini's celebrated one in the church of St. Petronius at Bologna, was eighty-three feet (French) high. But this mode of obtaining accuracy was soon abandoned for better methods. Three great improvements were introduced about the same time. The application of the micrometer to the telescope, by Huyghens, Malvasia, and Auzout; the application of the telescope to the astronomical quadrant, by Picard; and the fixation of the centre of its field by a cross of fine wires placed in the focus. We may judge how great was the improvement which these contrivances introduced into the art of observing, by finding that Hevelius refused to adopt them because they would make all the old observations of no value. He had spent a laborious and active life in the exercise of the old methods, and could not bear to think that all the treasures which he had accumulated had lost their worth by the discovery of a new mine of richer ore.

The apparent place of the object in the instrument being so precisely determined by the new methods, the exact division of the arc into degrees

and their subdivisions became a matter of great consequence. A series of artists, principally English, have acquired distinguished places in the lists of scientific fame by their performances in this way; and from that period, particular instruments have possessed historical interest and individual reputation. Graham was one of the first of these artists. He executed a great mural arc for Halley at Greenwich; for Bradley he constructed the sector which detected aberration. He also made the sector which the French academicians carried to Lapland; and probably the goodness of this instrument, compared with the imperfection of those which were sent to Peru, was one main cause of the great difference of duration in the two series of observations. Bird, somewhat later[1], (about 1750,) divided several quadrants for public observatories. His method of dividing was considered so perfect, that the knowledge of it was purchased by the English government, and published in 1767. Ramsden was equally celebrated. The error of one of his best quadrants (that at Padua) is said to be never greater than two seconds. But at a later period, Ramsden constructed mural circles only, holding this to be an instrument far superior to the quadrant. He made one of five feet diameter, in 1788, for M. Piazzi at Palermo; and one of eleven feet for the observatory of Dublin. Troughton, a worthy successor of the artists we have

[1] Mont. iv. 337.

mentioned, has invented a method of dividing the circle still superior to the former ones; indeed, one which is theoretically perfect, and practically capable of consummate accuracy. In this way, circles have been constructed for Greenwich, Armagh, Cambridge, and many other places; and probably this method, carefully applied, offers to the astronomer as much exactness as his other implements allow him to receive; but the slightest casualty happening to such an instrument, or any doubt whether the method of graduation has been rightly applied, make it unfit for the jealous scrupulosity of modern astronomy.

The English artists sought to attain accurate measurements by bisection and other aliquot subdivision of the limb of their circle; but Mayer proposed to obtain this end otherwise, by repeating the measure till the error of the instrument is unimportant, instead of attempting to make an instrument without error. This invention of the repeating circle was zealously adopted by the French, and the relative superiority of the rival methods is still a matter of difference of opinion.

2. *Clocks.*—The improvements in the measures of space require corresponding improvements in the measure of time. The beginning of anything which we can call accuracy, in this subject, was the application of the pendulum to clocks, by Huyghens, in 1656. That the successive oscillations of a pendulum occupy equal times, had been noticed by Galileo;

but in order to take advantage of this property, the pendulum must be connected with machinery by which its motion is kept from languishing, and the number of its swings recorded. By inventing such machinery, Huyghens at once obtained a measure of time more accurate than the sun itself. Hence astronomers were soon led to obtain the right ascension of a star, not directly, by measuring any distance in the heavens, but indirectly, by observing the moment of its transit. This observation is now made with a degree of accuracy which might, at first sight, appear beyond the limits of human sense, being noted to a *tenth of a second of time:* but we may explain this, by remarking that though the number of the second at which the transit happens is given by the clock, and is reckoned according to the course of time, the subdivision of the second of time into smaller fractions is performed by the eye,—by seeing the space described by the heavenly body in a whole second, and hence estimating a smaller time, according to the space which its description occupies.

But in order to make clocks so accurate as to justify this degree of precision, their construction was improved by various persons in succession. Picard soon found that Huyghens's clocks were affected in their going by temperature, for heat caused expansion of the metallic pendulum. This cause of error was remedied by combining different metals, as iron and copper, which expand in a different degree, in

such a way that their effects compensate each other. Graham afterwards used quicksilver for the same purpose. The escapement too, and other parts of the machinery, had the most refined mechanical skill and ingenuity of the best artists constantly bestowed upon them. The astronomer of the present day, constantly testing the going of such a clock by the motions of the fixed stars, has a scale of time as stable and as minutely exact as the scales on which he measures distance.

The construction of good watches, that is, portable or marine clocks, was important on another account, namely, because they might be used in determining the longitude of places. Hence the improvement of this little machine became an object of national interest, and was included in the reward of 20,000*l.* which we have already noticed as offered by the English parliament for the discovery of the longitude. Harrison[2], originally a carpenter, turned his mind to this subject with success. After thirty years of labour, in which he was encouraged by many eminent persons, he produced, in 1758, a time-keeper, which was sent on a voyage to Jamaica for trial. After 161 days, the error of the watch was only one minute five seconds, and the artist received from the nation 5000*l.* At a later period[3], at the age of seventy-five years, after a life devoted to this object, having still further satisfied the commissioners, he received, in 1765, 10,000*l.*, at the same time that

[2] Mont. iv. 554. [3] Ib. iv. 560.

Euler and the heirs of Mayer received each 3000*l.* for the lunar tables which they had constructed.

The two methods of finding the longitude, by chronometers and by lunar observations, have solved the problem for all practical purposes; but the latter could not have been employed at sea without the aid of that invaluable instrument, the sextant, in which the distance of two objects is observed, by bringing one to coincide apparently with the reflected image of the other. This instrument was invented by Hadley, in 1731. Though the problem of finding the longitude be, in fact, one of geography rather than astronomy, it is an application of astronomical science which has so materially affected the progress of our knowledge, that it deserves the notice we have bestowed upon it.

3. *Telescopes.*—We have spoken of the application of the telescope to astronomical measurements, but not of the improvement of the telescope itself. If we endeavour to augment the optical power of this instrument, we run, according to the path we take, into various inconveniences;—distortion, confusion, want of light, or coloured images. Distortion and confusion are produced, if we increase the magnifying power, retaining the length, and the aperture of the object-glass. If we diminish the aperture we suffer from loss of light. What remains then is to increase the focal length. This was done to an extraordinary extent, in telescopes constructed in the beginning of the last century. Huyghens, in his

first attempts, made them 22 feet long[4]; afterwards, Campani, by order of Louis the Fourteenth, made them of 86, 100, and 136 feet. Huyghens, by new exertions, made a telescope 210 feet long. Auzout and Hartsoecker are said to have gone much further, and to have succeeded in making an object-glass of 600 feet focus. But even such telescopes as those of Campani are almost unmanageable: in that of Huyghens, the object-glass was placed on a pole, and the observer was placed at the focus with an eye-glass.

The most serious objection to the increase of the aperture of object-glasses, was the coloration of the image produced, in consequence of the unequal refrangibility of differently coloured rays. Newton, who discovered the principle of this defect in lenses, had maintained that the evil was irremediable, and that a compound lens could no more refract without producing colour, than a single lens could. Euler and Klingenstierna doubted the exactness of Newton's proposition; and, in 1755, Dollond disproved it by experiment. This new light pointed out a method of making object-glasses which should give no colour;—which should be *achromatic*. For this purpose Dollond fabricated various kinds of glass (flint and crown glass;) and Clairaut and D'Alembert calculated formulæ. Dollond and his son[5] succeeded in constructing telescopes of three feet long (with a triple object-glass,) which produced an effect as great

[4] Bailly, ii. 253.

[5] Ib. iii. 118.

as those of forty-five feet on the ancient principles. At first it was conceived that these discoveries opened the way to a vast extension of the astronomer's power of vision; but it was found that the most material improvement was the compendious size of the new instruments; for, in increasing the dimensions, the optician was stopped by the impossibility of obtaining lenses of flint-glass of very large dimensions. And this branch of art remained long stationary; but, after a time, its epoch of advance again arrived. In the present century, Fraunhofer, in Germany, succeeded in forming lenses of flint-glass of a magnitude till then unheard of; and this art descended to his successors, Utzschneider in that country, and Guinand in Paris. Achromatic object-glasses, of a foot in diameter, and twenty feet focal length, are now no longer impossible; although in such attempts the artist cannot reckon on certain success.

Such telescopes might be expected to add something to our knowledge of the heavens, if they had not been anticipated by reflectors of an equal or greater scale. James Gregory had invented, and Newton had more efficaciously introduced, reflecting telescopes. But these were not used with any peculiar effect, till the elder Herschel made them his especial study. His skill and perseverance in grinding specula, and in contriving the best apparatus for their use, were rewarded by a number of curious and striking discoveries, among which, as we have

already related, was the discovery of a new planet beyond Saturn. In 1789, Herschel surpassed all his former attempts, by bringing into action a reflecting telescope of forty feet length, with a speculum of four feet in diameter. The first application of this magnificent instrument showed a new satellite (the sixth) of Saturn. He and his son have, with reflectors of twenty and of ten feet, made a complete survey of the heavens, so far as they are visible in this country; and the latter is now in a distant region completing this survey, by adding to it the other hemisphere.

In speaking of the improvements of telescopes we ought to notice, that they have been pursued in the eye-glasses as well as in the object-glasses. Instead of the single lens, Huyghens substituted an eye-piece of two lenses, which, though introduced for another purpose, attained the object of destroying colour[e]. Ramsden's eye-piece is one fit to be used with a micrometer, and others of more complex construction have been used for various purposes.

Sect. 2.—*Observatories.*

ASTRONOMY, which is thus benefited by the erection of large and stable instruments, requires also the establishment of permanent observatories, supplied with funds for their support, and for that of the observers. Such observatories have existed at all periods of the

[e] Coddington's Optics, ii. 21.

history of the science; but from the commencement of the period which we are now reviewing, they multiplied to such an extent that we cannot even enumerate them. Yet we must undoubtedly look upon such establishments, and the labours of which they have been the scene, as important and essential parts of the history of the progress of astronomy. Some of the most distinguished of the observatories of modern times we may mention. The first of these were that of Tycho Brahe at Uraniburg, and that of the Landgrave of Hesse Cassel at Cassel, where Rothman and Byrgius observed. But by far the most important observations, at least since those of Tycho which were the basis of the discoveries of Kepler and Newton, have been made at Paris and Greenwich. The observatory of Paris was built in 1667. It was there that the first Cassini made many of his discoveries; three of his descendants have since laboured in the same place, and two others of his family, the Maraldis[7]; besides many other eminent astronomers, as Picard, La Hire, Lefèvre, Fouchy, Legentil, Chappe, Méchain, Bouvard. Greenwich observatory was built a few years later (1675); and ever since its erection, the observations there made have been the foundation of the greatest improvements which astronomy, for the time, received. Flamsteed, Halley, Bradley, Bliss, Maskelyne, Pond, have occupied the place in succession; on the retirement of the last-named astronomer in 1835, Professor Airy was removed thither

[7] Mont. iv. 346.

from the Cambridge observatory. In every state, and in almost every principality in Europe, observatories have been established, but these have often fallen speedily into inaction, or have contributed little to the progress of astronomy, because their observations have not been published. From the same causes, the numerous private observatories which exist throughout Europe have added little to our knowledge, except where the attention of the astronomer has been directed to some definite points; as, for instance, the magnificent labours of the Herschels, or the skilful observations made by Mr. Pond with the Westbury circle, which first pointed out the error of graduation of the Greenwich quadrants. The observations, now regularly published*, are those of Greenwich, begun by Maskelyne, and continued quarterly by Mr. Pond; those of Königsberg, published by Bessel since 1814; of Vienna, by Littrow since 1820; of Speier, by Schwerd since 1826; those of Cambridge, commenced by Airy in 1828; of Armagh, by Robinson in 1829. Besides these, a number of useful observations have been published in journals and occasional forms; as, for instance, those of Zach, made at Seeberg, near Gotha, since 1788; and others have been employed in forming catalogues, of which we shall speak shortly.

Nor has the establishment of observatories been confined to Europe. In 1786, M. de Beauchamp, at the expense of Louis the Sixteenth, erected an

* Airy, Rep. p. 128.

observatory at Bagdad, "built to restore the Chaldean and Arabian observations," as the inscription stated; but probably, the restoration once effected, the main intention had been fulfilled, and little perseverance in observing was thought necessary. In 1828, the British government completed the building of an observatory at the Cape of Good Hope; and an observatory formed in New South Wales by Sir T. M. Brisbane in 1822, and presented by him to the government, is also in activity. The East India Company has founded observatories at Madras, Bombay, and St. Helena; and observations made at the former of these places, and at St. Helena, have been published.

The bearing of the work done at such observatories upon the past progress of astronomy, has already been seen in the preceding narrative. Their bearing upon the present condition of the science will be the subject of a few remarks hereafter.

Sect. 3.—*Scientific Societies.*

THE influence of scientific societies, or academical bodies, has also been very powerful in the subject before us. In all branches of knowledge, the use of such associations of studious and inquiring men is great; the clearness and coherence of a speculator's ideas, and their agreement with facts, (the two main conditions of scientific truth,) are severely but beneficially tested by collision with other minds. In

astronomy, moreover, the vast extent of the subject makes requisite the division of labour and the support of sympathy. The Royal Societies of London and of Paris were founded nearly at the same time as the metropolitan observatories of the two countries. We have seen what constellations of philosophers, and what activity of research, existed at those periods; these philosophers appear in the lists, their discoveries in the publications, of the above-mentioned eminent societies. As the progress of physical science, and principally of astronomy, attracted more and more admiration, Academies were created in other countries. That of Berlin was founded by Leibnitz in 1710; that of St. Petersburg was established by Peter the Great in 1725; and both these have produced highly-valuable Memoirs. In more modern times, these associations have multiplied almost beyond the power of estimation. They have been formed according to divisions, both of locality and of subject, conformable to the present extent of science, and the vast population of its cultivators. It would be useless to attempt to give a view either of their number or of the enormous mass of scientific literature which their Transactions present. But we may notice as specially connected with our present subject, the Astronomical Society of London, founded in 1820, which gave a strong impulse to the pursuit of the science in England.

Sect. 4.—*Patrons of Astronomy.*

THE advantages which letters and philosophy derive from the patronage of the great have sometimes been questioned; that love of knowledge, it has been thought, cannot be genuine which requires such stimulation, nor those speculations free and true which are thus forced into being. In the sciences of observation and calculation, however, in which disputed questions can be experimentally decided, and in which opinions are not disturbed by men's practical principles and interests, there is nothing necessarily operating to poison or neutralize the resources which wealth and power supply to the investigation of truth.

Astronomy has, in all ages, flourished under the favour of the rich and powerful; in the period of which we speak, this was eminently the case. Louis the Fourteenth gave to the astronomy of France a distinction which, without him, it could not have attained. No step perhaps tended more to this than his bringing the celebrated Dominic Cassini to Paris. This Italian astronomer (for he was born at Permaldo, in the county of Nice, and was professor at Bologna,) was already in possession of a brilliant reputation, when the French ambassador, in the name of his sovereign, applied to Pope Clement the Ninth, and to the senate of Bologna, that he should be allowed to remove to Paris. The request was

granted only so far as an absence of six years; but at the end of that time, the benefits and honours which the king had conferred upon him, fixed him in France. The impulse which his arrival (in 1669,) and his residence gave to astronomy, showed the wisdom of the measure. In the same spirit, the French government drew to Paris Römer from Denmark, Huyghens from Holland, and gave a pension to Hevelius, and a large sum when his observatory at Dantzic had been destroyed by fire in 1679.

When the sovereigns of Prussia and Russia were exerting themselves to encourage the sciences in their countries, they followed the same course which had been so successful in France. Thus, as we have said, the Czar Peter took Delisle to Petersburg in 1725; the celebrated Frederick the Great drew to Berlin, Voltaire and Maupertuis, Euler and Lagrange; and the Empress Catharine obtained in the same way Euler, two of the Bernoullis, and other mathematicians. In none of these instances, however, did it happen that "the generous plant did still its stock renew," as we have seen was the case at Paris, with the Cassinis, and their kinsmen the Maraldis.

It is not necessary to mention here the more recent cases in which sovereigns or statesmen have attempted to patronise individual astronomers.

Sect. 5.—Astronomical Expeditions.

BESIDES the pensions thus bestowed upon resident mathematicians and astronomers, the governments of Europe have wisely and usefully employed considerable sums upon expeditions and travels undertaken by men of science for some appropriate object. Thus Picard, in 1671, was sent to Uraniburg, the scene of Tycho's observations, to determine its latitude and its longitude. He found that "the city of the skies" had utterly disappeared from the earth; and even its foundations were retraced with difficulty. With the same object, that of accurately connecting the labours of the places which had been at different periods the metropolis of astronomy, Chazelles was sent, in 1693, to Alexandria. We have already mentioned Richer's astronomical expedition to Cayenne in 1672. Varin and Deshayes[9] were sent a few years later into the same regions for similar purposes. Halley's expedition to St. Helena in 1677, with the view of observing the southern stars, was at his own expense; but at a later period (in 1698,) he was appointed to the command of a small vessel by King William the Third, in order that he might make his magnetical observations in all parts of the world. La Caille was maintained by the French government four years at the Cape of Good Hope

[9] Bailly, ii. 374.

(1750-4,) for the purpose of observing the stars of the southern hemisphere. The two transits of Venus in 1761 and 1769, occasioned expeditions to be sent to Kamtschatka and Tobolsk by the Russians; to the Isle of France, and to Coromandel by the French[10]; to the isles of St. Helena and Otaheite by the English; to Lapland and to Drontheim, by the Swedes and Danes. I shall not here refer to the measures of degrees executed by various nations, still less the innumerable surveys by land and sea; but I may just notice the successive English expeditions of Captains Basil Hall, Sabine, and Foster, for the purpose of determining the length of the seconds' pendulum in different latitudes; and the voyages of Biot and others, sent by the French government for the same purpose. Much has been done in this way; but not more than the progress of astronomy absolutely required; and only a small portion of that which the completion of the subject calls for.

Sect. 6.—*Present State of Astronomy.*

ASTRONOMY, in its present condition, is not only much the most advanced of the sciences, but is also in far more favourable circumstances than any other science for making any future advance, as soon as this is possible. The general methods and conditions by which such an advantage is to be obtained

[10] Bailly, iii. 107.

for the various sciences, we shall endeavour hereafter to throw some light upon; but in the mean time, we may notice here some of the circumstances in which this peculiar felicity of the present state of astronomy may be traced.

The science is cultivated by a number of votaries, with an assiduity and labour, and with an expenditure of private and public resources, to which no other subject approaches; and the mode of its cultivation in all public and most private observatories has this character; that it forms, at the same time, a constant process of verification of existing discoveries, and a strict search for any new discoverable laws. The observations made are immediately referred to the best tables, and corrected by the best formulæ which are known; and if the result of such a reduction leaves anything unaccounted for, the astronomer is forthwith curious and anxious to trace this deviation from the expected numbers to its rule and its origin; and till the first, at least, of these things is performed, he is dissatisfied and unquiet. The reference of observations to the state of the heavens as known by previous researches, implies a great amount of calculation. The exact places of the stars at some standard period are recorded in *catalogues*; their movements, according to the laws hitherto detected, are arranged in *tables;* and if these tables are applied to predict the numbers which observation on each day ought to give, they form *ephemerides.* Thus the catalogues of fixed stars of

Flamsteed, of Piazzi, of Maskelyne, of the Astronomical Society, are the basis of all observation. To these are applied the corrections for refraction of Bradley or Bessel, and those for aberration, for nutation, for precession, of the best modern astronomers. The observations so corrected enable the observer to satisfy himself of the delicacy and fidelity of his measures of time and space; his clocks and his arcs. But this being done, different stars so observed can be compared with each other, and the astronomer can then endeavour further to correct his fundamental elements;—his catalogue, or his tables of corrections. In these Tables, though previous discovery has ascertained the law, yet the exact quantity, the *constant* or *coefficient* of the formula, can be exactly fixed only by numerous observations and comparisons. This is a labour which is still going on, and in which there are differences of opinion on almost every point; but the amount of these differences is the strongest evidence of the certainty and exactness of those doctrines in which all agree. Thus Lindenau makes the coefficient of nutation rather less than nine seconds, which other astronomers give as about nine seconds and three tenths. The Tables of Refraction are still the subject of much discussion, and of many attempts at improvement. And after or amid these discussions, arise questions whether there be not other corrections of which the law has not yet been assigned. The most remarkable example of such questions is the controversy concerning the

existence of an annual parallax of the fixed stars, which Brinkley asserted, and which Pond denied. Such a dispute between two of the best modern observers, only proves that the quantity in question, if it really exist, is of the same order as the hitherto unsurmounted errors of instruments and corrections. But besides the fixed stars and their corrections, the astronomer has the motions of the planets for his field of action. The established theories have given us tables of these, from which their daily places are calculated and given in our ephemerides, as the "Berliner Jahrbuch" of Encke, or the "Nautical Almanac," published by the government of this country. The comparison of the observed with the tabular place, gives us the means of correcting the coefficients of the tables; and thus of obtaining greater exactness in the constants of the solar system. But these constants depend upon the mass and form of the bodies of which the system is composed; and in this province, as well as in sidereal astronomy, different determinations, obtained by different paths, may be compared; and doubts may be raised and may be solved. In this way, the perturbations produced by Jupiter on different planets gave rise to a doubt whether his attraction be really proportional to his mass, as the law of universal gravitation asserts. The doubt has been solved by Nicolai and Encke in Germany, and by Airy in England. The mass of Jupiter, as shown by the perturbations of Juno, of Vesta, and of Encke's comet, and by the motion of

his outermost satellite, is found to agree, though different from the mass previously received on the authority of Laplace. Thus also Burckhardt, Littrow, and Airy, have corrected the elements of the solar tables. In other cases, the astronomer finds that no change of the coefficients will bring the tables and the observations to a coincidence;—that a new term in the formula is wanting. He obtains, as far as he can, the law of this unknown term; if possible, he traces it to some known or probable cause. Thus Airy, in his examination of the Solar Tables, not only found that a diminution of the received mass of Mars was necessary, but perceived discordances which led him to suspect the existence of a new inequality. Such an inequality was at length found to result theoretically from the attraction of Venus. Encke, in his examination of his comet, found a diminution of the periodic time in the successive revolutions; from which he inferred the existence of a resisting medium. Uranus still deviates from his tabular place, and the cause remains yet to be discovered.

Thus it is impossible that an assertion, false to any amount which the existing state of observation can easily detect, should have any abiding prevalence in astronomy. Such errors may long keep their ground in any science which is contained mainly in didactic works, and studied in the closet, but not acted upon elsewhere;—which is reasoned upon much, but brought to the test of experiment rarely or never. Here, on the contrary, an error, if it arise, makes its

way into the tables, into the ephemeris, into the observer's nightly list, or his sheet of reductions; the evidence of sense flies in its face in a thousand observatories; the discrepancy is traced to its source, and soon disappears for ever.

In this favoured branch of knowledge, the most recondite and delicate discoveries can no more suffer doubt or contradiction, than the most palpable facts of sense which the face of nature offers to our notice. The last great discovery in astronomy,—the motion of the stars arising from aberration,—is as obvious to the vast population of astronomical observers in all parts of the world, as the motion of the stars about the pole is to the casual night-wanderer. And this immunity from the danger of any large error in the received doctrines, is a firm platform on which the astronomer can stand and exert himself to reach perpetually further and further into the region of the unknown.

And this scrupulous care and diligence in recording all that has hitherto been ascertained, has been extended to other departments of astronomy in which, as yet, we have no general principles to bind together our acquired treasures. Thus, besides the catalogues of fundamental stars, we possess enormous assemblages of smaller stars, and other celestial objects. Flamsteed's "Historia Celestis," the greatest catalogue up to his time, contained 3000 stars. But in 1801, the French "Histoire Céleste" appeared, comprising observations of 50,000 stars. Catalogues or charts of other special portions of the

sky have been published more recently; and in 1825, the Berlin Academy proposed to the astronomers of Europe to carry on this work by portioning out the heavens among them. We have already said something of the observations of the two Herschels on double stars, which have led to acknowledgment of the law of the revolution of such systems. But besides these, the same illustrious astronomers have accumulated enormous treasures of observations of nebulæ; the materials, it may be, hereafter, of some vast new generalisation with respect to the history of the system of the universe.



BOOK VIII.

THE

SECONDARY MECHANICAL SCIENCES.

HISTORY OF ACOUSTICS.

. Ἐσσυμένη δὲ
Ἠερίην αψῖδα διερῥόιζησε πεδίλῳ
Εἰς δόμον ΑΡΜΟΝΙΗΣ παμμητόρος, ὁππόθι νύμφη
Ἴκελον οἶκον ἐνάιε τύπῳ τετράζυγι κόσμου
Αὐτοπαγῆ.

NONNUS. *Dionysiac.* xli. 275.

Along the skiey arch the goddess trode,
And sought Harmonia's august abode;
The universal plan, the mystic four,
Defines the figure of the palace-floor.
Solid and square the ancient fabric stands,
Raised by the labours of unnumbered hands.

INTRODUCTION.

The Secondary Mechanical Sciences.

In the sciences of Mechanics and Physical Astronomy, motion and force are the direct and primary objects of our attention. But there is another class of sciences in which we endeavour to reduce phenomena, not evidently mechanical, to a known dependence upon mechanical properties and laws. In the cases to which I refer, the facts do not present themselves to the senses as modifications of position and motion, but as *secondary qualities*, which are found to be in some way derived from those primary attributes. Also, in these cases, the phenomena are reduced to their mechanical laws and causes in a secondary manner; namely, by treating them as the operation of a *medium* interposed between the object and the organ of sense. These, then, we may call *Secondary Mechanical Sciences*. The sciences of this kind which require our notice are those which treat of the sensible qualities, sound, light, and heat; that is, Acoustics, Optics, and Thermotics.

It will be recollected that our object is not by any means to give a full statement of all the additions which have been made to our knowledge on the subjects under review, or a list of the persons by whom such additions have been made; but to present a

view of the progress of each of those branches of knowledge *as a speculative science*;—to point out the epochs of the discovery of those general principles which reduce many facts to one fact; and to note all that is most characteristic and instructive in the circumstances and persons which bear upon such epochs. A history of any science, written with such objects, will not need to be long; but it will fail in its purpose altogether, if it do not distinctly exhibit some well-marked and prominent features.

We begin our account of the Secondary Mechanical Sciences with Acoustics, because the progress towards right theoretical views, was, in fact, made much earlier in the science of sound, than in those of light and of heat; and also, because a clear comprehension of the theory to which we are led in this case, is the best preparation for the difficulties (by no means inconsiderable) of the reasonings of theorists on the other subjects.

CHAPTER I.

Prelude to the Solution of Problems in Acoustics.

In some measure the true theory of sound was guessed by very early speculators on the subject; though undoubtedly conceived in a very vague and wavering manner. That sound is caused by some motion of the sounding body, and conveyed by some motion of the air to the ear, is an opinion which we trace to the earliest times of physical philosophy. We may take Aristotle as the best expounder of this stage of opinion. In his Treatise *On Sound and Hearing*, he says, "Sound takes place when bodies strike the air, not by the air having a *form* impressed upon it, (σχηματιζόμενον,) as some think, but by its being moved in a corresponding manner; (probably he means in a manner corresponding to the impulse;) the air being contracted, and expanded, and overtaken, and again struck by the impulses of the breath and of the strings. For when the breath falls upon and strikes the air which is next it, the air is carried forwards with an impetus, and that which is contiguous to the first is carried onwards; so that the same voice spreads every way as far as the motion of the air takes place."

As is the case with all such specimens of ancient physics, different persons would find in such a state-

ment very different measures of truth and distinctness. The admirers of antiquity might easily, by pressing the language closely, and using the light of modern discovery, detect in this passage an exact account of the production and propagation of sound: while others might maintain that in Aristotle's own mind, there were only vague notions and verbal generalisations. This latter opinion is very emphatically expressed by Bacon[1]. "The collision or thrusting of air, which they will have to be the cause of sound, neither denotes the *form* nor the latent process of sound; but is a term of ignorance and of superficial contemplation." Nor can it be justly denied, that an exact and distinct apprehension of the kind of motion of the air by which sound is diffused, was beyond the reach of the ancient philosophers, and made its way into the world long afterwards. It was by no means easy to reconcile the nature of such motion with obvious phenomena. For the process is not evident as motion; since, as Bacon also observes[2], it does not visibly agitate the flame of a candle, or a feather, or any light floating substance, by which the slightest motions of the air are betrayed. Still, the persuasion that sound is some motion of the air, continued to keep hold of men's minds, and acquired additional distinctness. The illustration employed by Vitruvius, in the following passage, is even now one of the best we can offer[3]. "Voice is breath, flowing, and made sensible to

[1] Historia Soni et Auditus, vol. ix. p. 68. [2] Ibid.
[3] De Arch. v. 3.

the hearing by striking the air. It moves in infinite circumferences of circles, as when, by throwing a stone into still water, you produce innumerable circles of waves, increasing from the centre and spreading outwards, till the boundary of the space, or some obstacle, prevents their outlines from going further. In the same manner the voice makes its motions in circles. But in water the circles move breadthways upon a level plane; the voice proceeds in breadth, and also successively ascends in height."

Both the comparison, and the notice of the difference of the two cases, prove the architect to have had very clear notions on the subject; which he further shows by comparing the resonance of the walls of a building to the disturbance of the outline of the waves of water when they meet with a boundary and are thrown back. "Therefore, as in the outlines of waves in water, so in the voice, if no obstacle interrupt the foremost, it does not disturb the second and the following ones, so that all come to the ears of persons, whether high up or low down, without resonance. But when they strike against obstacles, the foremost, being thrown back, disturb the lines of those which follow." Similar analogies were employed by the ancients in order to explain the occurrence of echoes. Aristotle says[4], "An echo takes place, when the air, being as one body in consequence of the vessel which bounds it, and being prevented from being thrust forwards, is reflected

[4] De Animâ, ii. 8.

back like a ball." Nothing material was added to such views till modern times.

Thus the first conjectures of those who philosophized concerning sound, led them to an opinion concerning its causes and laws, which only required to be distinctly understood, and traced to mechanical principles, in order to form a genuine science of acoustics. It was, no doubt, a work which required a long time and sagacious reasoners, to supply what was thus wanting; but still, in consequence of this peculiar circumstance in the early condition of the prevalent doctrine concerning sound, the history of acoustics assumes a peculiar form. Instead of containing, like the history of astronomy or of optics, a series of generalisations, each including and rising above preceding generalisations; in this case, the highest generalisation is in view from the first; and the object of the philosopher is to determine its precise meaning and circumstances in each example. Instead of having a series of inductive truths, successively dawning on men's minds, we have a series of explanations, in which certain experimental facts and laws are reconciled, as to their mechanical principles and their measures, with the general doctrine already in our possession. Instead of having to travel gradually towards a great discovery, like universal gravitation, or luminiferous undulations, we take our stand upon acknowledged truths, the production and propagation of sound by the motion of bodies and of air; and we connect these with other truths, the laws of motion and the known properties

of bodies, as, for instance, their elasticity. Instead of *epochs of discovery*, we have *solutions of problems;* and to these we must now proceed.

We must, however, in the first place, notice that these problems include other subjects than the mere production and propagation of sound generally. For such questions as these obviously occur:—what are the laws and cause of the differences of sounds;—of acute and grave, loud and low, continued and instantaneous;—and, again, of the differences of articulate sounds, and of the quality of different voices and different instruments? The first of these questions, in particular, the real nature of the difference of acute and grave sounds, could not help attracting attention; since the difference of notes in this respect was the foundation of one of the most remarkable mathematical sciences of antiquity. Accordingly, we find attempts to explain this difference in the ancient writers on music. In Ptolemy's Harmonics, the third Chapter of the first Book is entitled, "How the acuteness and graveness of notes is produced;" and in this, after noting generally the difference of sounds, and the causes of difference, (which he states to be the force of the striking body, the physical constitution of the body struck, and other causes,) he comes to the conclusion, that "the things which produce acuteness in sounds, are a greater density and a smaller size; the things which produce graveness, are a greater rarity and a bulkier form." He afterwards explains this so as to include a considerable portion of truth. Thus he says, "that

in strings, and in pipes, other things remaining the same, those which are stopped at the smaller distance from the bridge give the most acute note; and in pipes, those notes which come through holes nearest to the mouth-hole are most acute." He even attempts a further generalisation, and says that the greater acuteness arises, in fact, from the body being more tense; and that thus "hardness may counteract the effect of greater density, as we see that brass produces a more acute sound than lead." But this author's notions of tension, since they were applied so generally as to include both the tension of a string, and the tension of a piece of solid brass, must necessarily have been very vague. And he seems to have been destitute of any knowledge of the precise nature of the motion or impulse by which sound is produced; and, of course, still more ignorant of the mechanical principles by which these motions are explained. The notion of *vibrations* of the parts of sounding bodies, does not appear to have been dwelt upon as an essential circumstance; though in some cases, as in sounding strings, the fact is very obvious. And the notion of vibrations of the air does not at all appear in ancient writers, except so far as it may be conceived to be implied in the comparison of aërial and watery waves, which we have quoted from Vitruvius. It is, however, very unlikely that, even in the case of water, the motions of the particles were distinctly conceived, for such conception is far from obvious.

The attempts to apprehend distinctly, and to

explain mechanically, the phenomena of sound, gave rise to a series of problems, of which we must now give a brief history. The questions which more peculiarly constitute the science of acoustics, are those concerning those affections of the air by which it is the medium of hearing. But the motions of sounding bodies have both so much connexion with those of the medium, and so much resemblance to them, that we shall include in our survey researches on that subject also.

CHAPTER II.

Problem of the Vibrations of Strings.

That the continuation of sound depends on a continued minute and rapid motion, a shaking or trembling, of the parts of the sounding body, was soon seen. Thus Bacon says[1], "The duration of the sound of a bell or a string when struck, which appears to be prolonged and gradually extinguished, does not proceed from the first percussion; but the trepidation of the body struck perpetually generates a new sound. For if that trepidation be prevented, and the bell or string be stopped, the sound soon dies: as in *spinets*, as soon as the *spine* is let fall so as to touch the string, the sound ceases." In the case of a stretched string, it is not difficult to perceive that the motion is a motion back and forwards across the straight line which the string occupies when at rest. The further examination of the quantitative circumstances of this oscillatory motion was an obvious problem; and especially after oscillations, though of another kind, (those of a pendulous body,) had attracted attention, as they had done in the school of Galileo. Mersenne, one of the promulgators of Galileo's philosophy in France, is the first author in whom I find an examination of the details

[1] Hist. Son. et Aud. vol. ix. p. 71.

of this case, (*Harmonicorum Liber*, Paris, 1636.) He asserts[1], that the differences and concords of acute and grave sounds depend on the rapidity of vibrations, and their ratio; and he proves this doctrine by a series of experimental comparisons. Thus he finds[2] that the note of a string is as its length, by taking a string first twice, and then four times as long as the original string, other things remaining the same. This, indeed, was known to the ancients, and was the basis of that numerical indication of the notes which the proposition expresses. Mersenne further proceeds to show the effect of thickness and tension. He finds (Prop. 7) that a string must be four times as thick as another, to give the octave below; he finds, also (Prop. 8), that the tension must be about four times as great in order to produce the octave above. From these proportions various others are deduced, and the *law of the phenomena* of this kind may be considered as determined. Mersenne also undertook to *measure* the phenomena numerically, that is, to determine the number of vibrations of the string in each of such cases; which at first might appear difficult, since it is obviously impossible to count with the eye the passages of a sounding string backwards and forwards. But Mersenne rightly assumed, that the number of vibrations is the same so long as the tone is the same, and that the ratios of the numbers of vibrations of different strings may be determined from the numerical relations of their notes. He

[1] L. i. Prop. 15. [2] L. ii. Prop. 6.

had, therefore, only to determine the number of vibrations of one certain string, or one known note, to know those of all others. He took a musical string of three quarters of a foot long, stretched with a weight of six pounds and five-eighths, which he found gave him by its vibrations a certain standard note in his organ: and finding that a string of the same material and tension, fifteen feet, that is, twenty times as long, made ten recurrences in a second, he inferred that the number of vibrations must also be twenty times as great; and thus such a string must make in one second of time two hundred vibrations.

This determination of Mersenne does not appear to have attracted due notice; but some time afterwards attempts were made to ascertain the connexion between the sound and its elementary pulsations in a more direct manner. Hooke, in 1681, produced sounds by the striking of the teeth of brass wheels[4], and Stancari, in 1706, by whirling round a large wheel in air, showed, before the academy of Bologna, how the number of vibrations in a given note might be known. Sauveur, who, though deaf for the first seven years of his life, was one of the greatest promoters of the science of sound, and gave it its name of *acoustics*, endeavoured also, about the same time, to determine the number of vibrations of a standard note, or, as he called it, fixed sound. He employed two methods, both ingenious and both indirect. The first was the method of *beats*. Two organ-pipes,

[4] Life, p. xxiii.

which form a discord, are often heard to produce a kind of *howl*, or *wavy* noise, the sound swelling and declining at small intervals of time. This was readily and rightly ascribed to the coincidences of the pulsations of sound of the two notes after certain cycles. Thus, if the number of vibrations of the notes were as fifteen to sixteen, every fifteenth vibration of the one would coincide with every sixteenth vibration of the other, while all the intermediate vibrations of the two tones would, in various degrees, disagree with each other; and thus every such cycle, of fifteen or sixteen vibrations, might be heard as a separate beat of sound. Now, Sauveur wished to take a case in which these beats were so slow as to be counted[5], and in which the ratio of the vibrations of the notes was known from a knowledge of their musical relations. Thus if the two notes form an interval of a semitone, their ratio will be that above supposed, fifteen to sixteen; and if the beats be found to be six in a second, we know that, in that time, the graver note makes ninety and the acuter ninety-six vibrations. In this manner Sauveur found that an open organ-pipe, five feet long, gave one hundred vibrations in a second.

Sauveur's other method is more recondite, and approaches to a mechanical view of the question[6]. He proceeded on this basis; a string, horizontally stretched, cannot be drawn into a mathematical straight line, but always hangs in a very flat curve,

[5] Ac. Sc. Hist. 1700, p. 131. [6] Ib. 1713.

or *festoon*. Hence Sauveur assumed, that its transverse vibrations may be conceived to be identical with the lateral swingings of such a festoon. Observing that the string C, in the middle of a harpsichord, hangs in such a festoon to the amount of 1-323rd of an inch, he calculates, by the laws of pendulums, the time of oscillation, and finds it 1-122nd of a second. Thus this C, his *fixed note*, makes one hundred and twenty-two vibrations in a second. It is curious that this process, seemingly so arbitrary, is capable of being justified on mechanical principles; though we can hardly give the author credit for the views which this justification implies. It is, therefore, easy to understand that it agreed with other experiments in the laws which it gave for the dependence of the tone on the length and tension.

The problem of satisfactorily explaining this dependence, on mechanical principles, naturally pressed upon the attention of mathematicians when the law of the phenomena was thus completely determined by Mersenne and Sauveur. It was desirable to show that both the circumstances and the measure of the phenomena were such as known mechanical causes and laws would explain. But this problem, as might be expected, was not attacked till mechanical principles, and the modes of applying them, had become tolerably familiar.

As the vibrations of a string are produced by its tension, it appeared to be necessary, in the first place, to determine the law of the tension which is called *into action* by the motion of the string; for it is

manifest that, when the string is drawn aside from the straight line into which it is stretched, there arises an additional tension, which aids in drawing it back to the straight line as soon as it is let go. Hooke (*On Spring*, 1678) determined the law of this additional tension, which he expressed in his noted formula, "Ut tensio sic vis," the force is as the tension; or rather, to express his meaning more clearly, the force of tension is as the extension, or, in a string, as the increase of length. But, in reality, this principle, which is important in many acoustical problems, is, in the one now before us, unimportant; the force which urges the string towards the straight line, depends, with such small extensions as we have now to consider, not on the extension, but on the curvature; and the power of treating the mathematical difficulty of curvature, and its mechanical consequences, was what was requisite for the solution of this problem.

The problem, in its proper aspect, was first attacked and mastered by Brook Taylor, an English mathematician of the school of Newton, by whom the solution was published in 1715, in his *Methodus Incrementorum*. Taylor's solution was indeed imperfect, for it only pointed out a form and a mode of vibration, with which the string *might* move consistently with the laws of mechanics; not the mode in which it *must* move, supposing its form to be any whatever. It showed that the curve might be of the nature of that which is called *the companion to the cycloid;* and, on the supposition of the curve of

the string being of this form, the calculation confirmed the previously established laws by which the tone, or the time of vibration, had been discovered to depend on the length, tension, and bulk of the string. The mathematical incompleteness of Taylor's reasoning must not prevent us from looking upon his solution of the problem as the most important step in the progress of this part of the subject: for the difficulty of applying mechanical principles to the question being once overcome, the extension and correction of the application was sure to be undertaken by succeeding mathematicians; and, accordingly, this soon happened. We may add, moreover, that the subsequent and more general solutions, require to be considered with reference to Taylor's, in order to apprehend distinctly their import; and further, that it was almost evident to a mathematician, even before the general solution had appeared, that the dependence of the time of vibration on the length and tension, would be the same in the general case as in the Taylorian curve; so that, for the ends of physical philosophy, the solution was not very incomplete.

John Bernoulli, a few years afterwards[7], solved the problem of vibrating chords on nearly the same principles and suppositions as Taylor; but a little later (in 1747), the next generation of great mathematicians, D'Alembert, Euler, and Daniel Bernoulli, applied the increased powers of analysis to give

[7] Op. iii. p. 207.

generality to the mode of treating this question, and especially the calculus of partial differentials, invented for this purpose. But at this epoch, the discussion, so far as it bore on physics, belonged rather to the history of another problem, which comes under our notice hereafter, that of the composition of vibrations; we shall, therefore, defer the further history of the problem of vibrating strings, till we have to consider it in connexion with new experimental facts.

CHAPTER III.

Problem of the Propagation of Sound.

We have seen that the ancient philosophers, for the most part, held that sound was transmitted, as well as produced, by some motion of the air, without defining what kind of motion; that some, however, applied to it a very happy similitude, the expansive motion of the circular waves produced by throwing a stone into still water; but that notwithstanding, some rejected this mode of conception, as, for instance, Bacon, who ascribed the transmission of sound to certain "spiritual species."

Though it was an obvious thought to ascribe the motion of sound to some motion of air; to conceive what kind of motion could and did produce this effect, must have been a matter of grave perplexity at the time of which we are speaking; and is far from easy to most persons even now. We may judge of the difficulty of forming this conception, when we recollect that John Bernoulli the younger[1] declared, that he could not understand Newton's proposition on this subject. The difficulty consists in this, that the movement of the parts of air, in which sound consists, travels along, but that the parts of air themselves do not so travel. Accordingly Otto Guericke[2], the

[1] *Prize Dis.* on Light, 1736. [2] De Vac. Spat. p. 138.

inventor of the air-pump, asks, "How can sound be conveyed by the motion of the air? when we find that it is better conveyed through air that is still, than when there is a wind." We may observe, however, that he was partly misled by finding, as he thought, that a bell could be heard in the vacuum of his air-pump; a result which arose, probably, from some imperfection in his apparatus.

Attempts were made to determine, by experiment, the circumstances of the motion of sound; and especially its velocity. Gassendi[3] was one of the first who did this. He employed fire-arms for the purpose, and thus found the velocity to be 1473 Paris feet in a second. Roberval found a velocity so small (560 feet) that it threw uncertainty upon the rest, and affected Newton's reasonings subsequently[4]. Cassini, Huyghens, Picard, Römer, found a velocity of 1172 Paris feet, which is more accurate than the former. Gassendi had been surprised to find that the velocity with which sounds travel, is the same whether they are loud or gentle.

The explanation of this constant velocity of sound, and of its amount, was one of the problems of which a solution was given in the Great Charter of modern science, Newton's Principia (1687). There, for the first time, were explained the real nature of the motions and mutual action of the parts of the air through which sound is transmitted. It was shown[5]

[3] Fischer. Gesch. d. Physik. vol. i. 171.

[4] Newt. Princ. B. ii. P. 50, Schol. [5] Ib. B. ii. P. 43.

that a body vibrating in an elastic medium, will propagate *pulses* through the medium; that is, the parts of the medium will move forwards and backwards, and this motion will affect successively those parts which are at a greater and greater distance from the origin of motion. The parts, in going forwards, produce condensation; in returning to their first places, they allow extension; and the play of the elasticities developed by these expansions and contractions, supplies the forces which continue to propagate the motion.

The idea of such a motion as this, is, as we have said, far from easy to apprehend distinctly: but a distinct apprehension of it is a step essential to the physical part of the sciences now under notice; for it is by means of such *pulses*, or *undulations*, that not only sound, but light, and probably heat, are propagated. We constantly meet with evidence of the difficulty which men have in conceiving this undulatory motion, and in separating it from a local motion of the medium as a mass. For instance, it is not easy at first to conceive the waters of a great river flowing constantly *down* towards the sea, while waves are rolling *up* the very same part of the stream, and while the great elevation, which makes the tide, is travelling from the sea perhaps with a velocity of fifty miles an hour. The motion of such a wave, or elevation, is distinct from any stream, and is of the nature of undulations in general. The parts of the fluid stir for a short time and for a small distance, so as to accumulate themselves on a neighbouring part,

and then retire to their former place; and this movement affects the parts in the order of their places. Perhaps if the reader looks at a field of standing corn when the gusts are sweeping over it in visible waves, he will have his conception of this matter aided; for he will see that here, where each ear of grain is anchored by its stalk, there can be no permanent local motion of the substance, but only a successive stooping and rising of the separate straws, producing hollows and waves, closer and laxer strips of the crowded ears.

Newton had, moreover, to consider the mechanical consequences which such condensations and rarefactions of the elastic medium, air, would produce in the parts of the fluid itself. Employing known laws of the elasticity of air, he showed, in a very remarkable proposition[4], the law according to which the particles of air might vibrate. We may observe, that in this solution, as in that of the vibrating string already mentioned, a rule was exhibited according to which the particles *might* oscillate, but not the law to which they *must* conform. It was proved that, by taking the motion of each particle to be perfectly similar to that of a pendulum, the forces, developed by contraction and expansion, were precisely such as the motion required; but it was not shown that no other type of oscillation would give rise to the same accordance of force and motion. Newton's reasoning also gave a determination of the speed of propagation

[4] Princ. B. ii. Prop. 48.

of the pulses: it appeared that sound ought to travel with the velocity which a body would acquire by falling freely through half *the height of a homogeneous atmosphere;* "the height of a homogeneous atmosphere" being the height which the air must have, in order to produce, at the earth's surface, the actual atmospheric pressure, supposing no diminution of density to take place in ascending. This height is about 29,000 feet; and hence it followed that the velocity was 968 feet. This velocity is really considerably less than that of sound, but at the time of which we speak, no accurate measure had been established; and Newton persuaded himself, by experiments made in the cloister of Trinity College, his residence, that his calculation was not far from the fact. When, afterwards, more exact experiments showed the velocity to be 1142 English feet, Newton attempted to explain the difference by various considerations, none of which were adequate to the purpose;—as the dimensions of the solid particles of which the fluid air consists;—or the vapours which are mixed with it. Other writers offered other suggestions; but the true solution of the difficulty was reserved for a period considerably subsequent.

Newton's calculation of the motion of sound, though logically incomplete, was the great step in the solution of the problem; for mathematicians could not but presume that his result was not restricted to the hypothesis on which he had obtained it; and the extension of the solution required only

mere ordinary talents. The logical defect of his solution was assailed, as might have been expected. Cramer (professor at Geneva), in 1741, conceived that he was destroying the conclusiveness of Newton's reasoning, by showing that it applied equally to other modes of oscillation. This, indeed, contradicted the enunciation of the 48th Prop. of the Second Book of the Principia; but it confirmed and extended all the general results of the demonstration; for it left even the velocity of sound unaltered, and thus showed that the velocity did not depend mechanically on the type of the oscillation. But the satisfactory establishment of this physical generalisation was to be supplied from the vast generalisations of analysis, which mathematicians were now becoming able to deal with. Accordingly this task was performed by the great master of analytical generalisation, Lagrange, in 1759, when, at the age of twenty-three, he and two friends published the first volume of the Turin Memoirs. Euler, as his manner was, at once perceived the merit of the new solution, and pursued the subject on the views thus suggested. Various analytical improvements and extensions were introduced into the solution by the two great mathematicians; but none of these at all altered the formula by which the velocity of sound was expressed; and the discrepancy between calculation and observation, about one-sixth of the whole, which had perplexed Newton, remained still unaccounted for.

The merit of satisfactorily explaining this discre-

pancy belongs to Laplace. He was the first to remark[7] that the common law of the changes of elasticity in the air, as dependent on its compression, cannot be applied to those rapid vibrations in which sound consists, since the sudden compression produces a degree of heat which additionally increases the elasticity. The ratio of this increase depended on the experiments by which the relation of heat and air is established. Laplace, in 1816, published[8] the theorem on which the correction depends. On applying it, the calculated velocity of sound agreed very closely with the best antecedent experiments, and was confirmed by more exact ones instituted for that purpose.

This step completes the solution of the problem of the propagation of sound, as a mathematical induction obtained from, and verified by, facts. Most of the discussions concerning points of analysis to which the investigations on this subject gave rise, as, for instance, the admissibility of *discontinuous functions* into the solutions of partial differential equations, belong to the history of pure mathematics. Those which really concern the physical theory of sound may be referred to the problem of the motion of air in tubes, to which we shall soon have to proceed; but we must first speak of another form which the problem of vibrating strings assumed.

[7] Méc. Cél. t. v. l. xii. p. 96. [8] Ann. Phys. et Chim. T. iii. p. 288.

CHAPTER IV.

Problem of Different Sounds of the same String.

It had been observed at an early period of acoustical knowledge, that one string might give several sounds. Mersenne and others had noticed[1] that when a string vibrates, one which is in unison with it vibrates without being touched. He was also aware that this was true if the second string was an octave or a twelfth below the first. This was observed as a new fact in England in 1674, and communicated to the Royal Society by Wallis[2]. But the later observers ascertained further, that the longer string divides itself into two, or into three equal parts, separated by *nodes* or points of rest; this they proved by hanging bits of paper on different parts of the string. The discovery so modified was again made by Sauveur[3] about 1700. The sounds thus produced in one string by the vibration of another, have been termed *Sympathetic Sounds*. Similar sounds are often produced by performers on stringed instruments, by touching the string at one of its aliquot divisions, and are then called the *Acute Harmonics*. Such facts were not difficult to explain on Taylor's view of the mechanical condition of the

[1] Harm. lib. iv. Prop. 28, (1636.) [2] Ph. Tr. 1677, April.
[3] A. P. 1701.

string; but the difficulty was increased when it was noticed that a sounding body could produce these different notes *at the same time*. Mersenne had remarked this, and the fact was more distinctly observed and pursued by Sauveur. The notes thus produced in addition to the genuine note of the string, have been called *secondary notes*; those usually heard are, the octave, the twelfth, and the seventeenth above the note itself. To supply a mode of conceiving distinctly, and explaining mechanically, vibrations which should allow of such an effect, was therefore a requisite step in acoustics.

This task was performed by Daniel Bernoulli in a memoir published in 1755[4]. He there stated and proved the principle of *the coexistence of small vibrations*; it was already established, that a string might vibrate either in a single *swelling* (if we use this word to express the curve between two nodes which Bernoulli calls a *ventre*,) or in two or three or any number of equal swellings with immoveable nodes between. Daniel Bernoulli showed further, that these nodes might be combined, each taking place as if it were the only one. This appears sufficient to explain the coexistence of the harmonic sounds just noticed. D'Alembert, indeed, in the article *Fundamental* in the French Encyclopédie, and Lagrange in his Dissertation on Sound in the Turin Memoirs[5], offer several objections to this explanation; and it cannot be denied that the subject has

[4] Berlin Mem. 1753, p. 147.

[5] T. i, p. 64, 103.

its difficulties; but still these do not deprive Bernoulli of the merit of having pointed out the principle of coexistent vibrations, or divest that principle of its value in physical science.

Daniel Bernoulli's Memoir, of which we speak, was published at a period when the clouds which involve the general analytical treatment of the problem of vibrating strings, were thickening about Euler and D'Alembert, and darkening into a controversial hue; and as Bernoulli ventured to interpose his view, as a solution of these difficulties, which, in a mathematical sense, it is not, we can hardly be surprised that he met with a rebuff. The further prosecution of the different modes of vibration of the same body need not be here considered.

The sounds which are called *Grave Harmonics*, have no analogy with the acute harmonics abovementioned; nor do they belong to this section; for in the case of grave harmonics, we have one sound from the co-operation of two strings, instead of several sounds from one string. These harmonics are, in fact, connected with beats, of which we have already spoken; the beats becoming so close as to produce a note of definite musical quality. The discovery of the grave harmonics is usually ascribed to Tartini, who mentions them in 1754; but they are first noticed[6] in the work of Sorge *On tuning Organs*, 1744. He there expresses this discovery in a query. "Whence comes it, that if we tune a fifth 2:3, a

[6] Chladni. Acoust. p. 254.

third sound is faintly heard, the octave below the lower of the two notes? Nature shows that with 2 : 3, she still requires the unity to perfect the order 1, 2, 3." The truth is, that these numbers express the frequency of the vibrations, and thus there will be coincidences of the notes 2 and 3, which are of the frequency 1, and consequently give the octave below the sound 2. This is the explanation given by Lagrange[7], and is indeed obvious.

[7] Mem. Tur. i. p. 104.

CHAPTER V.

Problem of the Sounds of Pipes.

It was taken for granted by those who reasoned on sounds, that the sounds of flutes, organ-pipes, and wind-instruments in general, consisted in vibrations of some kind; but to determine the nature and laws of these vibrations, and to reconcile them with mechanical principles, was far from easy. The leading facts which had been noticed were, that the note of a pipe was proportional to its length, and that a flute and similar instruments might be made to produce some of the acute harmonics, as well as the genuine note. It had further been noticed[1], that pipes closed at the end, instead of giving the series of harmonics 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., would give only those notes which answer to the odd numbers 1, $\frac{1}{3}$, $\frac{1}{5}$, &c. In this problem also, Newton[2] made the first step to the solution. At the end of the propositions respecting the velocity of sound, of which we have spoken, he noticed that it appeared by taking Mersenne's or Sauveur's determination of the number of vibrations corresponding to a given note, that the pulse of air runs over twice the length of the pipe in the time of each vibration. He does not follow out this observation, but it obviously points to the

[1] D. Bernoulli, Berlin Mem. 1753, p. 150.

[2] Princip. Schol. Prop. 50.

theory, that the sound of a pipe consists of pulses which travel back and forwards along its length, and are kept in motion by the breath of the player. This supposition would account for the observed dependence of the note on the length of the pipe. The subject does not appear to have been again taken up in a theoretical way till about 1760; when Lagrange in the second volume of the Turin Memoirs, and D. Bernoulli in the Memoirs of the French Academy for 1762, published important essays, in which some of the leading facts were satisfactorily explained, and which may therefore be considered as the principal solutions of the problem.

In these solutions there was necessarily something hypothetical. In the case of vibrating strings, as we have seen, the form of the vibrating curve was guessed at only, but the existence and position of the nodes could be rendered visible to the eye. In the vibrations of air, we cannot see either the places of nodes, or the mode of vibration; but several of the results are independent of these circumstances. Thus both of the solutions explain the fact, that a tube closed at one end is in unison with an open tube of double the length; and, by supposing nodes to occur, they account for the existence of the odd series of harmonics alone, 1, 3, 5, in closed tubes, while the whole series, 1, 2, 3, 4, 5, &c., occurs in open ones. Both views of the nature of the vibration appear to be nearly the same; though Lagrange's is expressed with an analytical generality which renders it obscure, and Bernoulli

has perhaps laid down an hypothesis more special than was necessary. Lagrange[3] considers the vibrations of open flutes as "the oscillations of a fibre of air," under the condition that its elasticity at the two ends is, during the whole oscillation, the same as that of the surrounding atmosphere. Bernoulli supposes[4] the whole inertia of the air in the flute to be collected into one particle, and this to be moved by the whole elasticity arising from its displacement. It may be observed that both these modes of treating the matter come very near to what we have stated as Newton's theory; for though Bernoulli supposes all the air in the flute to be moved at once, and not successively, as by Newton's pulse, in either case the whole elasticity moves the whole air in the tube, and requires more time to do this according to its quantity. Since that time, the subject has received further mathematical developement from Euler[5], Lambert[6], and Poisson[7]; but no new explanation of facts has arisen. Attempts have however been made to ascertain experimentally the places of the nodes. Bernoulli himself had shown that this place was affected by the amount of opening, and Lambert[8] had examined other cases with the same view. Savart traced the node in various musical pipes under different conditions; and very recently, Mr. Hopkins, of Cambridge, has pursued the same

[3] Mém. Turin, vol. ii. p. 154.
[4] Mém. Berlin, 1753, p. 446.
[5] Nov. Act. Petrop. tom. xvi.
[6] Acad. Berlin, 1775.
[7] Journ. Ec. Polyt. cap. 14.
[8] Acad. Berlin, 1775.

experimental inquiry[9]. It appears from these researches, that the early assumptions of mathematicians with regard to the position of the nodes, are not justified by the facts. When the air in a pipe is made to vibrate so as to have several nodes which divide it into equal parts, it had been supposed by acoustical writers that the part adjacent to the open end was half of the other parts; the outermost node, however, is found experimentally to be *displaced* from the position thus assigned to it, by a quantity depending on several collateral circumstances.

Since our purpose was to consider this problem only so far as it has tended towards a mathematical solution, we have avoided saying anything of the dependence of the mode of vibration on the cause by which the sound is produced; and consequently, the researches on the effects of reeds, embouchures, and the like, by Chladni, Savart, Willis and others, do not belong to our subject. It is easily seen that the complex effect of the elasticity and other properties of the reed and of the air together, is a problem of which we can hardly hope to give a complete solution till our knowledge has advanced much beyond its present condition.

Indeed in the science of acoustics there is a vast body of facts to which we might apply what has just been said; but for the sake of pointing out some of them, we shall consider them as the subjects of one extensive and yet unsolved problem.

[9] Camb. Trans. vol. v. p. 234.

CHAPTER VI.

Problem of different Modes of Vibration of Bodies in general.

Not only the objects of which we have spoken hitherto, strings and pipes, but almost all bodies are capable of vibration. Bells, gongs, tuning-forks, are examples of solid bodies; drums and tambourines of membranes; if we run a wet finger along the edge of a glass goblet, we throw the fluid which it contains into a regular vibration; and the various character which sounds possess according to the room in which they are uttered, shows that large masses of air have peculiar modes of vibration. Vibrations are generally accompanied by sound, and they may, therefore, be considered as acoustical phenomena, especially as the sound is one of the most decisive facts in indicating the mode of vibration. Moreover, every body of this kind can vibrate in many different ways, the vibrating segments being divided by nodal lines and surfaces of various form and number. The mode of vibration, selected by the body in each case, is determined by the way in which it is held, the way in which it is set in vibration, and the like circumstances.

The general problem of such vibrations includes the discovery and classification of the phenomena; the detection of their formal laws; and, finally, the

explanation of these on mechanical principles. We must speak very briefly of what has been done in these ways. The facts which indicate nodal lines had been remarked by Galileo, on the sounding-board of a musical instrument; and Hooke had proposed to observe the vibrations of a ball by strewing flour upon it. But it was Chladni, a German philosopher, who enriched acoustics with the discovery of the vast variety of symmetrical figures of nodal lines, exhibited on plates of regular forms, when made to sound. His first investigations on this subject, *Entdeckungen über die Theorie des Klangs*, were published in 1787: and in 1802 and 1817 he added other discoveries. In these works he not only related a vast number of new and curious facts, but in some measure reduced some of them to order and law. For instance, he has traced all the vibrations of square plates to a resemblance with those forms of vibration in which there are nodal lines, parallel to one side of the square and to the other; and he has established a notation for the modes of vibration founded on this classification. Thus, 5-2 denotes a form in which there are five nodal lines parallel to one side, and two to another; or a form which can be traced to a disfigurement of such a standard type. Savart pursued this subject still further; and traced, by actual observation, the forms of the nodal surfaces which divide solid bodies, and masses of air, when in a state of vibration.

The dependence of such vibrations upon their *physical* cause, the elasticity of the substance, we

can conceive in a general way; but the mathematical theory of such cases, is, as might be supposed, very difficult, even if we confine ourselves to the obvious question of the mechanical possibility of these different modes of vibration, and leave out of consideration their dependence upon the mode of excitation. The transverse vibrations of elastic rods, plates, and rings, had been considered by Euler in 1779; but his calculations concerning plates had foretold only a small part of the curious phenomena observed by Chladni[1]; and the several notes which, according to his calculation, the same ring ought to give, were not in agreement with experiment[2]. Indeed, researches of this kind, as conducted by Euler, and other authors[3], rather were, and were intended for, examples of analytical skill, than explanations of physical facts. James Bernoulli, after the publication of Chladni's experiments in 1787, attempted to solve the problem for plates, by treating a plate as a collection of fibres; but, as Chladni observes, the justice of this mode of conception is disproved, by the disagreement of its results with experiment.

The Institute of France, which had approved of Chladni's labours, proposed, in 1809, the problem now before us as a prize-question[4]:—"To give the mathematical theory of the vibrations of elastic surfaces, and to compare it with experiment." Only one memoir was sent in as candidate for the prize;

[1] Fischer, vi. 587.
[2] Ib. vi. 596.
[3] See Chladni, p. 474.
[4] Ib. p. 357.

and this was not crowned, though honourable mention was made of it[5]. The formulæ of James Bernoulli were, according to M. Poisson's statement, defective, in consequence of his not taking into account the normal force which acts at the exterior boundary of the plate[6]. The author of the anonymous memoir corrected this error, and calculated the note corresponding to various figures of the nodal lines; and he found an agreement with experiment sufficient to justify his theory. He had not, however, proved his fundamental equation, which M. Poisson demonstrated in a Memoir, read in 1814[7]. At a more recent period also, MM. Poisson and Cauchy (as well as a lady, Mlle. Sophie Germain,) have applied to this problem the artifices of the most improved analysis. M. Poisson[8] determined the relation of the notes given by the longitudinal and the transverse vibrations of a rod; and solved the problem of vibrating circular plates when the nodal lines are concentric circles. In both these cases, the numerical agreement of his results with experience, seemed to confirm the justice of his fundamental views[9]. He proceeds upon the hypothesis, that elastic bodies are composed of separate particles held together by the attractive forces which they exert upon each other, and distended by the repulsive force of heat. M. Cauchy[10] has also calculated the

[5] Poisson's Mém. in Ac. Sc. 1812, p. 169. [6] Ib. p. 220.
[7] Ib. 1812, p. 2. [8] Ib. t. viii. 1829.
[9] An. Chim. tom. xxxvi. 1827, p. 90.
[10] Exercices de Mathématique, iii. and iv.

transverse, longitudinal, and rotatory vibrations of elastic rods, and has obtained results agreeing closely with experiment through a considerable list of comparisons. The combined authority of two profound analysts, as MM. Poisson and Cauchy are, leads us to believe that, for the simpler cases of the vibrations of elastic bodies, mathematics has executed her task; but most of the more complex cases remain as yet unsubdued.

The two brothers, Ernest and William Weber, made many curious observations on undulations, which are contained in their "Wellenlehre," (Doctrine of Waves,) published at Leipsig in 1825. They were led to suppose, (as Young had suggested at an earlier period,) that Chladni's figures of nodal lines in plates were to be accounted for by the superposition of undulations[11]. Mr. Wheatstone[12] has undertaken to account for Chladni's figures of vibrating *square* plates by this superposition of two or more simple and obviously allowable modes of nodal division, which have the same time of vibration. He assumes, for this purpose, certain "primary figures," containing only *parallel* nodal lines; and by combining these, first in twos, and then in fours, he obtains most of Chladni's observed figures, and accounts for their transitions and deviations from regularity.

The principle of the superposition of vibrations is so solidly established as a mechanical truth, that we

[11] Wellenlehre, p. 474.

[12] Phil. Trans. 1833, p. 593.

may consider an acoustical problem as satisfactorily disposed of, when it is reduced to that principle, as well as when it is solved by analytical mechanics: but at the same time we may recollect, that the right application and limitation of this law involves no small difficulty; and in this case, as in all advances in physical science, we cannot but wish to have the new ground which has been gained, gone over by some other person in some other manner; and thus secured to us as a permanent possession.

Savart's Laws.—In what has preceded, the vibrations of bodies have been referred to certain general classes, the separation of which was suggested by observation; for example, the *transverse*, *longitudinal*, and *rotatory*[18], vibrations of rods. The transverse vibrations, in which the rod goes backwards and forwards across the line of its length, were the only ones noticed by the earlier acousticians: the others were principally brought into notice by Chladni. As we have already seen in the preceding pages, this classification serves to express important laws; as for instance, a law obtained by M. Poisson which gives the relation of the notes produced by the transverse and longitudinal vibrations of a rod. But this distinction was employed by M. Felix Savart to express laws of a more general kind; and then, as often happens in the progress of science, by pursuing these laws to a higher point of generality, the dis-

[18] Vibrations tournantes.

tinction again seemed to vanish. A very few words will explain these steps.

It was long ago known that vibrations may be communicated by contact. The distinction of transverse and longitudinal vibrations being established, Savart found that if one rod touch another perpendicularly, the longitudinal vibrations of the first occasion transverse vibrations in the second, and *vice versâ*. This is the more remarkable, since the two sets of vibrations are not equal in rapidity, and therefore cannot sympathise in any obvious manner[14]. Savart found himself able to generalise this proposition, and to assert that in any combination of rods, strings, and laminæ, at right angles to each other, the longitudinal and transverse vibrations affect respectively the rods in the one and other direction[15], so that when the horizontal rods, for example, vibrate in the one way, the vertical rods vibrate in the other.

This law was thus expressed in terms of that classification of vibrations of which we have spoken. Yet we easily see that we may express it in a more general manner, without referring to that classification, by saying, that vibrations are communicated so as always to be parallel to their original direction. And by following it out in this shape by means of experiment, M. Savart was led, a short time afterwards, to deny that there is any essential distinction in these different kinds of vibration. "We are

[14] An. Chim. 1819, tom. xiv. p. 138.
[15] Ib. p. 152.

thus led," he says[16] in 1822, "to consider *normal* [transverse] vibrations as only one circumstance in a more general motion common to all bodies, analogous to *tangential* [longitudinal and rotatory] vibrations; that is, as produced by small *molecular oscillations*, and differently modified according to the direction which it affects, relatively to the dimensions of the vibrating body."

These "inductions," as he properly calls them, are supported by a great mass of ingenious experiments; and may be considered as well-established, when they are limited to molecular oscillations, employing this phrase in the sense in which it is understood in the above statement; and also when they are confined to bodies in which the play of elasticity is not interrupted by parts more rigid than the rest, as the sound-post of a violin[17]. And before I quit the subject, I may notice a consequence which M. Savart has deduced from his views, and which, at first sight, appears to overturn most of the earlier doctrines respecting vibrating bodies. It was formerly held that tense strings and elastic rods could vibrate only in a determinate series of modes of division, with no intermediate steps. But M. Savart maintains[18], on the contrary, that they produce sounds which are gradually transformed into one another, by indefinite intermediate degrees.

[16] An. Chim. t. xxv. p. 33.

[17] For the suggestion of the necessity of this limitation I am indebted to Mr. Willis.

[18] An. Chim. 1826, t. xxxii. p. 384.

The reader may naturally ask, what is the solution of this apparent contradiction between the earliest and the latest discoveries in acoustics. And the answer must be, that these intermediate modes of vibration are complex in their nature, and difficult to produce; and that those which were formerly believed to be the only possible vibrating conditions, are so eminent above all the rest by their features, their simplicity, and their facility, that we may still, for common purposes, consider them as a class apart; although for the sake of reaching a general theorem, we may associate them with the general mass of cases of molecular vibrations. And thus we have no exception here, as we can have none in any case, to our maxim, that what formed part of the early discoveries of science, forms part of its latest systems.

We have thus surveyed the progress of the science of sound up to recent times, with respect both to the discovery of laws of phenomena, and the reduction of these to their mechanical causes. The former branch of the science has necessarily been inductively pursued; and therefore has been more peculiarly the object of our attention. And this consideration will explain why we have not dwelt more upon the deductive labours of the great analysts who have treated of this problem.

To those who are acquainted with the high and deserved fame which the labours of D'Alembert, Euler, Lagrange, and others, upon this subject, enjoy among mathematicians, it may seem as if we had

not given them their due prominence in our sketch. But it is to be recollected here, as we have already observed in the case of hydrodynamics, that even when the general principles are uncontested, mere mathematical deductions from them do not belong to the history of physical science, except when they point out laws which are intermediate between the general principle and the individual facts, and which observation may confirm.

The business of constructing any science may be figured as the task of forming a road on which our reason can travel through a certain province of the external world. We have to throw a bridge which may lead from the chambers of our own thoughts, from our speculative principles, to the distant shore of material facts. But in all cases the abyss is too wide to be crossed, except we can find some intermediate points on which the piers of our structure may rest. Mere facts, without connexion or law, are only the rude stones hewn from the opposite bank, of which our arches may, at some time, be built. But mere hypothetical mathematical calculations are only plans of projected structures; and those plans which exhibit only one vast and single arch, or which suppose no support but that which our own position supplies, will assuredly never become realities. We must have a firm basis of intermediate generalisations in order to frame a continuous and stable edifice.

In the subject before us, we have no want of such points of intermediate support, although they are in

many instances irregularly distributed and obscurely seen. The number of observed laws and relations of the phenomena of sound, is already very great; and though the time may be distant, there seems to be no reason to despair of one day uniting them by clear ideas of mechanical causation, and thus of making acoustics a perfect secondary mechanical science.

The historical sketch just given includes only such parts of acoustics as have been in some degree reduced to general laws and physical causes; and thus excludes much that is usually treated of under that head. Moreover, many of the numerical calculations connected with sound belong to its agreeable effect upon the ear; as the properties of the various systems of *temperament.* These are parts of theoretical music, not of acoustics;—of the philosophy of the fine arts, not of physical science; and may be referred to in a future portion of this work, so far as they bear upon our object.

The science of acoustics may, however, properly consider other differences of sound than those of acuto and grave,—for instance, the *articulate* differences, or those by which the various letters are formed. Some progress has been made in reducing this part of the subject to general rules; for though Kempelen's "talking machine" was only a work of art, Mr. Willis's machine[19], which exhibits the rela-

[19] On the vowel sounds, and on reed organ-pipes. Camb. Tr. iii. 237.

tion among the vowels, gives us a law such as forms a step in science. We may, however, consider this instrument as a *phthongometer*, or measure of vowel quality; and in that point of view we shall have to refer to it again when we come to speak of such measures.

BOOK IX.

SECONDARY MECHANICAL SCIENCES.

(CONTINUED.)

HISTORY OF OPTICS,

FORMAL AND PHYSICAL.

Ὦ Διὸς ὑψιμέλαθρον ἔχων κράτος ἀιὲν ἀτειρὲς
Ἄστρων, Ἠελίου τε, Σεληναίης τε μέρισμα
Πανδαμάτωρ, πυρίπνου, πᾶσιν ζωοῖσιν ἔνασμα
Τφιφάνης ΑΙΘΗΡ, κόσμου στοιχεῖον ἄριστον·
Ἀγλαὸν ὦ βλάστημα, σελασφόρον, ἀστεροφεγγὲς
Κικλήσκων λίτομαι σε, κεκραμένον ὄυδιον εἶναι.

ORPHEUS. HYMN.

O thou who fillest the palaces of Jove;
Who flowest round moon, and sun and stars above;
Pervading, bright, life-giving element,
Supernal ETHER, fair and excellent;
Fountain of hope and joy, of light and day,
We own at length thy tranquil, steady sway.

INTRODUCTION.

Formal and Physical Optics.

The history of the science of Optics, written at length, would be very voluminous; but we shall not need to make our history so; since our object is only to illustrate the nature of science and the conditions of its progress. In this way Optics is peculiarly instructive; the more so, as its history has followed a course in some respects different from both the sciences previously reviewed. Astronomy, as we have seen, advanced with a steady and continuous movement from one generation to another, from the earliest time, till her career was crowned by the great unforeseen discovery of Newton; Acoustics had her extreme generalisation in view from the first, and her history consists in the correct application of it to successive problems; Optics advanced through a scale of generalisations as remarkable as that of astronomy; but for a long period she was almost stationary; and, at last, was rapidly impelled through all those stages by the energy of two or three discoverers. The highest point of generality which Optics has reached is little different from that which Acoustics occupied at once; but in the older and earlier science we still want that palpable and pointed confirmation of the

general principle, which the undulatory theory receives from optical phenomena. Astronomy has amassed her vast fortune by long-continued industry and labour; Optics has obtained hers in a few years by sagacious and happy speculations; Acoustics, having early acquired a competence, has since been employed rather in improving and adorning than in extending her estate.

The successive inductions by which Optics made her advances, might, of course, be treated in the same manner as those of astronomy, each having its prelude and its sequel. But most of the discoveries in Optics are of a smaller character, and have less employed the minds of men, than those of astronomy; and it will not be necessary to exhibit them in this detailed manner, till we come to the great generalisation by which the theory was established. I shall, therefore, now pass rapidly in review the earlier optical discoveries, without any such division of the series.

Optics, like Astronomy, has for its object of inquiry, first, the laws of phenomena, and next, their causes; and we may hence divide this science, like the other, into *Formal Optics* and *Physical Optics*. The distinction is clear and substantive, but it is not easy to adhere to it in our narrative; for, after the theory had begun to make its rapid advance, many of the laws of phenomena were studied and discovered in immediate reference to the theoretical cause, and do not occupy a separate place in the history of science, as in astronomy they do. We

may add, that the reason why Formal Astronomy was almost complete before Physical Astronomy began to exist, was, that it was necessary to construct the science of Mechanics in the mean time, in order to be able to go on; whereas, in Optics, mathematicians were able to calculate the results of the undulatory theory as soon as it had suggested itself from the earlier facts, and while the great mass of facts were only becoming known.

We shall, then, in the first *nine* chapters of the History of Optics, treat of the Formal Science, that is, the discovery of the laws of phenomena. The classes of phenomena which will thus pass under our notice are numerous; namely, reflection, refraction, chromatic dispersion, achromatization, double refraction, polarization, dipolarization, the colours of thin plates, the colours of thick plates, and the fringes and bands which accompany shadows. All these cases had been studied, and, in most of them, the laws had been in a great measure discovered, before the physical theory of the subject gave to our knowledge a simpler and more solid form.

FORMAL OPTICS.

CHAPTER I.

PRIMARY INDUCTION OF OPTICS.—RAYS OF LIGHT AND LAWS OF REFLECTION.

IN speaking of the Ancient History of Physics, we have already noticed that the optical philosophers of antiquity had satisfied themselves that vision is performed in straight lines;—that they had fixed their attention upon those straight lines, or *rays*, as the proper object of the science;—they had ascertained that rays reflected from a bright surface make the *angle of reflection* equal to the *angle of incidence;*—and they had drawn several consequences from these principles.

We may add to the consequences already mentioned, the art of *perspective*, which is merely a corollary from the doctrine of rectilinear visual rays; for if we suppose objects to be referred by such rays to a plane interposed between them and the eye, all the rules of perspective follow directly. The ancients practised this art, as we see in the pictures which remain to us; and we learn from Vitruvius[1], that they also wrote upon it. Agatharchus, who had

[1] De Arch. ix. Mont. i. 707.

been instructed by Eschylus in the art of making decorations for the theatre, was the first author on this subject, and Anaxagoras, who was a pupil of Agatharchus, also wrote an *Actinographia*, or doctrine of drawing by rays: but none of these treatises are come down to us. The moderns re-invented the art in the flourishing times of their painting, that is, about the end of the fifteenth century; and, belonging to that period also, we have treatises[1] upon it.

But these are only deductive applications of the most elementary optical doctrines; we must proceed to the inductions by which further discoveries were made.

[1] Gauricus, 1504.

CHAPTER II.

Discovery of the Law of Refraction.

It being once clearly apprehended that vision is performed by rectilinear rays, there were many facts which indicated the kind of bending which such rays undergo at the surfaces of transparent bodies, although it required some geometrical precision and steadiness of conception to trace their course distinctly. Accordingly, the notice of such facts during the stationary period is confused and unconnected. Seneca[1] remarks that an oar in clear water appears broken; and that apples seen through a glass are magnified. But on these phenomena he merely makes the vague reflection that "nothing is so fallacious as our sight;" and does not seem aware that the form of the glass has any effect on the appearance. We can hardly doubt, however, that some of the ancients had more exact views; for Archimedes is said to have published a book "On a Ring seen under Water;" and he was certainly not a person to be content with indistinct geometrical ideas. The term *refraction* (ἀνακλάσις) had been used, though in a vague manner, by Aristotle[2]. It appears to be more technically introduced in the "Optical Hypo-

[1] Nat. Quæst. x. lib. i. c. 3.

[2] Meteorol. iii. 2. "The vision is refracted, as by water, so *by air, and by* all bodies which have the surface smooth."

theses" of Heliodorus Larissæus; but in this work, as we have it, there is no account of the phenomena of refraction. The first sound views occur in the Arabian mathematician, Alhazen, who lived about 1100; in this author it is asserted (lib. vii.), that "refraction takes place towards the perpendicular;" and reference is made to experiment for the proof. On the same ground he states that the quantities of refraction differ according to the magnitudes of the angles which the (*primæ lineæ*) directions of incidence make with the perpendicular to the surface; and moreover (which shows accuracy as well as distinctness,) that the angles of refraction do not follow the proportion of the angles of incidence.

After reaching this point, there remained nothing to be done with regard to refraction, except to go on experimenting and conjecturing till the true law of refraction was discovered, and, in the mean time, to apply the principle as far as it was known. The latter task was, in part, performed by Alhazen himself, who shows, in a manner almost correct, how a line is magnified by being seen under water. In Roger Bacon's works we find a tolerably distinct explanation[3] of the effect of a convex glass; and in the work of Vitellio, a Pole, who, like Bacon, flourished in the thirteenth century, the effect of refraction at the two surfaces of a glass globe is clearly traced[4].

The rule which determines the amount of refraction was naturally the next object of curiosity; and

[3] Opus Magnum, p. 343. [4] Optica, p. 443.

this point attracted more interest, in consequence of the introduction of a correction for atmospheric refraction into astronomical calculations by Tycho, and of the invention of the telescope. Vitellio had obtained experimentally[5] a number of measures of the refraction out of air into water and into glass. Out of these facts no rule had yet been collected, when, in 1604[6], Kepler published his "Supplement to Vitellio." The reader who recollects what we have already narrated, the manner in which Kepler attempted to reduce to law the astronomical observations of Tycho,—devising an almost endless variety of possible formulæ, tracing their consequences with undaunted industry, and relating, with a vivacious garrulity, his disappointments and his hopes,—will not be surprised to find that he proceeded in the same manner with regard to Vitellio's Tables of Observed Refractions. He tried a variety of constructions by triangles, conic sections, &c., without being able to satisfy himself; and he at last[7] is obliged to content himself with an approximate rule, which makes the refraction partly proportional to the angle of incidence, and partly, to the secant of that angle. In this way he satisfies the observed refractions within a difference of less than half a degree each way. When we consider how simple the law of refraction is, (that the ratio of the sines of the angles of incidence and refraction is constant for the same medium,) it appears strange that a person attempting

[5] Optica, p. 411.
[6] L. U. K. Life of Kepler, p. 14.
[7] Ib. p. 115.

to discover it, and drawing triangles for the purpose, should fail; but this lot of missing what afterwards seems to have been obvious, is a common one in the pursuit of truth.

The person who did discover the Law of the Sines, was Willebrord Snell, about 1621; but the law was first published by Descartes, who had seen Snell's papers[8]. Descartes does not acknowledge this law to have been first detected by another; and after his manner, instead of establishing its reality by reference to experiment, he pretends to prove *à priori* that it must be true[9], comparing, for this purpose, the particles of light, to balls striking a substance which *accelerates* them.

But though Descartes does not, in this instance, produce any good claims to the character of an inductive philosopher, he showed considerable skill in tracing the consequences of the principle when once adopted. In particular we must consider him as the genuine author of the explanation of the rainbow. It is true, that Fleischer[10] and Kepler had previously ascribed this phenomenon to the rays of sunlight which, falling on drops of rain, are refracted into each drop, reflected at its inner surface, and refracted out again: Antonio de Dominis had found that a glass globe of water, when placed in a particular position with respect to the eye, exhibited bright colours; and had hence explained the circular form of the bow, which, indeed, Aristotle had done

[8] Huyghens, Dioptrica, p. 2. [9] Diopt., p. 53.
[10] Mont. i. 701.

before[11]. But none of these writers had shown why there was a narrow bright circle of a certain definite diameter; for the drops which send rays to the eye after two refractions and a reflection, occupy a much wider space in the heavens. Descartes assigned the reason for this in the most satisfactory manner[12], by showing that the rays which, after two refractions and a reflection, come to the eye at an angle of about forty-one degrees with their original direction, are far more dense than those in any other position. He showed, in the same manner, that the existence and position of the secondary bow resulted from the same laws. This is the complete and adequate account of the state of things, so far as the brightness of the bows only is concerned; the explanation of the colours belongs to the next article of our survey.

The explanation of the rainbow and of its magnitude, afforded by Snell's law of sines, was perhaps one of the leading points in the verification of the law. The principle, being once established, was applied, by the aid of mathematical reasoning, to atmospheric refractions, optical instruments, *diacaustic* curves, (that is, the curves of intense light produced by refraction,) and to various other cases; and was, of course, tested and confirmed by such applications. It was, however, impossible to pursue these applications far, without a due knowledge of the laws by which, in such cases, colours are produced. To these we now proceed.

[11] *Meteorol.* iii. 3. [12] Meteorum, cap. viii, p. 196.

CHAPTER III.

Discovery of the Law of Dispersion by Refraction.

Early attempts were made to account for the colours of the rainbow, and various other phenomena in which colours are seen to arise from transient and unsubstantial combinations of media. Thus Aristotle explains the colours of the rainbow by supposing[1] that it is light seen through a dark medium: "Now," says he, "the bright seen through the dark appears red, as, for instance, the fire of green wood seen through the smoke, and the sun through mist. Also[2] the weaker is the light, or the visual power, and the nearer the colour approaches to the black; becoming first red, then green, then purple. But[3] the vision is strongest in the outer circle, because the periphery is greater;—thus we shall have a gradation from red, through green, to purple, in passing from the outer to the inner circle." This account would hardly have deserved much notice, if it had not been for a strange attempt to revive it, or something very like it, in modern times. The same doctrine is found in the work of De Dominis[4]. According to him, light is white: but if we mix with the light something

[1] Meteor. iii. 3. p. 373. [2] Ib. p. 374. [3] Ib. p. 375.
[4] Cap. iii. p. 9. See also Göthe Farbenl. vol. ii. p. 251.

dark, the colours arise,—first red, then green, then blue or violet. He applies this to explain the colours of the rainbow[5], by means of the consideration that, of the rays which come to the eye from the globes of water, some go through a larger thickness of the globe than others, whence he obtains the gradation of colours just described.

Descartes came far nearer the true philosophy of the iridal colours. He found that a similar series of colours was produced by refraction of light bounded by shade, through a prism[6]; and he rightly inferred that neither the curvature of the surface of the drops of water, nor the reflection, nor the repetition of refraction, were necessary to the generation of such colours. In further examining the course of the rays, he approaches very near to the true conception of the case; and we are led to believe that he might have anticipated Newton in his discovery of the unequal refrangibility of different colours, if it had been possible for him to reason any otherwise than in the terms and notions of his preconceived hypotheses. The conclusion which he draws is[7], that "the particles of the subtile matter which transmit the action of light, endeavour to rotate with so great a force and impetus, that they cannot move in a straight line (whence comes refraction): and that those particles which endeavour to revolve much more strongly produce a red colour, those which endeavour to move

[5] Göthe, p. 263. [6] Meteor. Sect. viii. p. 190.
[7] Sect. vii. p. 192.

only a little more strongly produce yellow." Here we have a clear perception that colours and unequal refraction are connected, though the cause of refraction is expressed by a gratuitous hypothesis. And we may add, that he applies this notion rightly, so far as he explains himself[8], to account for the colours of the rainbow.

It appears to me that Newton and others have done Descartes injustice, in ascribing to De Dominis the true theory of the rainbow. There are two main points of this theory, namely, the showing that a *bright* circular band, of a certain definite diameter, arises from the great intensity of the light returned at a certain angle; and the referring the different colours to the *different quantity of the refraction;* and both these steps appear indubitably to be the discoveries of Descartes. And he informs us that these discoveries were not made without some exertion of thought. "At first," he says[9], "I doubted whether the iridal colours were produced in the same way as those in the prism; but, at last, taking my pen, and carefully calculating the course of the rays which fall on each part of the drop, I found that many more come at an angle of forty-one degrees, than either at a greater or a less angle. So that there is a bright bow terminated by a shade; and hence the colours are the same as those produced through a prism."

The subject was left nearly in the same state, in

[8] Meteor. Sect. ix. [9] Sect. ix. p. 193.

the work of Grimaldi, *Physico-Mathesis, de Lumine, Coloribus et Iride*, published at Bologna in 1665. There is in this work a constant reference to numerous experiments, and a systematic exposition of the science in an improved state. The author's calculations concerning the rainbow are put in the same form as those of Descartes; but he is further from seizing the true principle on which its coloration depends. He rightly groups together a number of experiments in which colours arise from refraction[10]; and explains them by saying that the colour is brighter where the light is denser: and the light is denser on the side from which the refraction turns the ray, because the increments of refraction are greater in the rays that are more inclined[11]. This way of treating the question might be made to give a sort of explanation of most of the facts, but is much more erroneous than a developement of Descartes's view would have been.

At length, in 1672, Newton gave[12] the true explanation of the facts; namely, that light consists of rays of different colours and different refrangibility. This now appears to us so obvious a mode of interpreting the phenomena, that we can hardly understand how they can be conceived in any other manner; but yet the impression which this discovery made, both upon Newton and upon his contemporaries, shows how remote it was from the then

[10] Prop. 35, p. 254. [11] Ib. p. 256.

[12] Phil. Trans. t. vii. p. 3075.

accepted opinions. There appears to have been a general persuasion that the coloration was produced, not by any peculiarity in the law of refraction itself, but by some collateral circumstance,—some dispersion or variation of density of the light, in addition to the refraction. Newton's discovery consisted in teaching distinctly that the law of refraction was to be applied, not to the beam of light in general, but to the colours in particular.

When Newton produced a bright spot on the wall of his chamber, by admitting the sun's light through a small hole in his window-shutter, and making it pass through a prism, he expected the image to be round; which, of course, it would have been, if the colours had been produced by an equal dispersion in all directions; but to his surprise he saw the image, or *spectrum*, five times as long as broad. He found that no consideration of the different thickness of the glass, the possible unevenness of its surface, or the different angles of rays proceeding from the two sides of the sun, could be the cause of this shape. He found, also, that the rays did not go from the prism to the image in curves; he was then convinced that the different colours were refracted separately, and at different angles; and he confirmed this opinion by transmitting and refracting the rays of each colour separately.

The experiments are so easy and common, and Newton's interpretation of them so simple and evident, that we might have expected it to receive general assent; indeed, as we have shown, Descartes

had already been led very near the same point. In fact, Newton's opinions were not long in obtaining general acceptance; but they met with enough of cavil and misapprehension to annoy extremely the discoverer, whose clear views and quiet temper made him impatient alike of stupidity and of contentiousness.

We need not dwell long on the early objections which were made to Newton's doctrine. A Jesuit, of the name of Ignatius Pardies, professor at Clermont, at first attempted to account for the elongation of the image, by the difference of the angles made by the rays from the two edges of the sun, which would produce a difference in the amount of refraction of the two borders; but when Newton pointed out the calculations which showed the insufficiency of this explanation, he withdrew his opposition. Another more pertinacious opponent appeared in Francis Linus, a physician of Liege; who maintained, that having tried the experiment, he found the sun's image, when the sky was clear, to be round and not oblong; and he ascribed the elongation noticed by Newton, to the effect of clouds. Newton for some time refused to reply to this contradiction of his assertions, though obstinately persisted in; and his answer was at last sent, just about the time of Linus's death, in 1675. But Gascoigne, a friend of Linus, still maintained that he and others had seen what the Dutch physician had described; and Newton, who was pleased with the candour of Gascoigne's letter, suggested that the Dutch experimenters might have taken one of the

images reflected from the surfaces of the prism, of which there are several, instead of the proper refracted one. By the aid of this hint, Lucas of Liege repeated Newton's experiments, and obtained Newton's result, except that he never could obtain a spectrum whose length was more than three and a half times its breadth. Newton, on his side, persisted in asserting that the image would be five times as long as broad, if the experiment were properly made. It is curious that he should have been so confident of this, as to conceive himself certain that such would be the result in all cases. We now know that the dispersion, and consequently the length, of the spectrum, is very different for different kinds of glass, and it is very probable that the Dutch prism was really less dispersive than the English one[13]. The erroneous assumption which Newton made in this instance, he held by to the last; and was thus prevented from making the discovery of which we have next to speak.

Newton was attacked by persons of more importance than those we have yet mentioned; namely, Hooke and Huyghens. These philosophers, however, did not object so much to the laws of refraction of different colours, as to some expressions used by Newton, which, they conceived, conveyed false notions respecting the composition and nature of light. Newton had asserted that all the different colours are of

[13] Brewster's Newton, p. 50.

distinct kinds, and that, by their composition, they form white light. This is true of colours as far as their analysis and composition by refraction are concerned; but Hooke maintained that all natural colours are produced by various combinations of two primary ones, red and violet[14]; and Huyghens held a similar doctrine, taking, however, yellow and blue for his basis. Newton answers, that such compositions as they speak of, are not compositions of simple colours in his sense of the expressions. These writers also had both of them adopted an opinion that light consisted in vibrations; and objected to Newton that his language was erroneous, as involving the hypothesis that light was a body. Newton appears to have had a horror of the word hypothesis, and protests against its being supposed that his "theory" rests on such a foundation.

The doctrine of the unequal refrangibility of different rays is clearly exemplified in the effects of lenses, which produce images more or less bordered with colour, in consequence of this property. The improvement of telescopes was, in Newton's time, the great practical motive for aiming at the improvement of theoretical optics. Newton's theory showed why they were imperfect, and was confirmed by the circumstances of such imperfections. The false opinion of which we have already spoken, that the dispersion must be the same when the refraction is the same, led him to believe that the imperfection was insur-

[14] Brewster's Newton, p. 54. Phil. Trans. viii. 5084, 6086.

mountable, and made him turn his attention to the construction of reflecting instead of refracting telescopes. But the rectification of Newton's error was a further confirmation of the general truth of his principles in other respects; and since that time, the soundness of the Newtonian law of refraction has hardly been questioned among physical philosophers.

It has, however, in modern times, been very vehemently controverted in a quarter from which we might not readily have expected a detailed discussion on such a subject. The celebrated Göthe has written a work on *The Doctrine of Colours*, (Farbenlehre; Tübingen, 1810,) one main purpose of which is, to represent Newton's opinions and the work in which they are formally published (his *Opticks*,) as utterly false and mistaken, and capable of being assented to only by the most blind and obstinate prejudice. Those who are acquainted with the extent to which such an opinion, promulgated by Göthe, was likely to be widely adopted in Germany, will not be surprised that similar language is used by other writers of that nation. Thus Schelling[15] says, "Newton's Opticks is the greatest proof of a whole structure of fallacies, which, in all its parts, is founded upon observation and experiment." Göthe, however, does not concede even so much to Newton's work. He goes over a large portion of it, page by page, and quarrels with experiments, diagrams, reasoning, and language, without intermission; and holds that it is

[15] Vorlesungen, p. 270.

not reconcileable with the most simple facts. He declares[16], that the first time he looked through a prism, he saw the white walls of the room still look white, "and though alone, I pronounced, as by an instinct, that the Newtonian doctrine is false." We need not here point out how inconsistent with the Newtonian doctrine it was, to expect, as Göthe expected, that the wall should be all over coloured various colours.

Göthe not only adopted and strenuously maintained the opinion that the Newtonian theory was false, but he framed a system of his own to explain the phenomena of colour. As a matter of curiosity, it may be worth our while to state the nature of this system; although undoubtedly it forms no part of the progress of physical science. Göthe's views are, in fact, little different from those of Aristotle and Antonio de Dominis, though more completely and systematically developed. Colours arise when we see through a dim medium ("ein trübes mittel"). Light in itself is colourless; but if it be seen through a somewhat dim medium, it appears yellow; if the dimness of the medium increases, or if its depth be augmented, we see the light gradually assume a yellow-red colour, which finally is heightened to a ruby-red. On the other hand, if darkness is seen through a dim medium which is illuminated by a light falling on it, a blue colour is seen, which becomes clearer and paler, the more the dimness of

[16] Farbenlehre, vol. ii. p. 678.

the medium increases, and darker and fuller, as the medium becomes more transparent; and when we come to "the smallest degree of the purest dimness," we see the most perfect violet[17]. In addition to this "doctrine of the dim medium," we have a second principle asserted concerning refraction. In a vast variety of cases, images are accompanied by "accessory images," as when we see bright objects in a looking-glass[18]. Now, when an image is displaced by refraction, the displacement is not complete, clear and sharp, but incomplete, so that there is an accessory image along with the principal one[19]. From these principles, the colours produced by refraction in the image of a bright object on a dark ground, are at once derivable. The accessory image is semitransparent[20]; and hence that border of it which is pushed forwards, is drawn from the dark over the bright, and there the yellow appears; on the other hand, where the clear border laps over the dark ground, the blue is seen[21]; and hence we easily see that the image must appear red and yellow at one end, and blue and violet at the other.

We need not explain this system further, or attempt to show how vague and loose, as well as baseless, are the notions and modes of conception which it introduces. Perhaps it is not difficult to point out the peculiarities in Göthe's intellectual character which led to his singularly unphilosophical

[17] Farbenlehre, § 150, p. 151. [18] Ib. § 223.
[19] Ib. § 227. [20] Ib. § 238. [21] Ib. § 239.

views on this subject. One important circumstance is, that he appears, like many persons in whom the poetical imagination is very active, to have been destitute of the talent and the habit of geometrical thought. In all probability, he never apprehended clearly and steadily those relations on which the Newtonian doctrine depends. Another cause of his inability to accept the doctrine probably was, that he had conceived the "composition" of colours in some way altogether different from that which Newton understands by composition. What Göthe expected to see, we cannot clearly collect; but we know, from his own statement, that his intention of experimenting with a prism arose from his speculations on the rules of colouring in pictures; and we can easily see that any notion of the composition of colours which such researches would suggest, would require to be laid aside, before he could understand Newton's theory of the composition of light.

Other objections to Newton's theory, of a kind very different, have been recently made by Sir David Brewster. He contests Newton's opinion, that the coloured rays into which light is separated by refraction are altogether simple and homogeneous, and incapable of being further analysed or modified. For he finds that by passing such rays through coloured media, (as blue glass for instance,) they are not only absorbed and transmitted in very various degrees, but that some of them have their colour altered; which cannot be conceived otherwise than a further analysis of them, one component colour

being absorbed and the other transmitted[22]. And on this subject we can only say, as we have before said, that Newton has incontestibly and completely established his doctrine, so far as analysis and decomposition *by refraction* are concerned; but that with regard to any other analysis, which absorbing media or other agents may produce, we have no right from his experiments to assert, that the colours of the spectrum are incapable of decomposition. The whole subject of the colours of objects, both opake and transparent, is still in obscurity. Newton's conjectures concerning the causes of the colours of natural bodies, appear to help us little; and his opinions on that subject are to be separated altogether from the important step which he made in optical science, by the establishment of the true doctrine of refractive dispersion.

We now proceed to the corrections which the next generation introduced into the details of this doctrine.

[22] This latter fact has, however, been denied by other experimenters.

CHAPTER IV.

Discovery of Achromatism.

The discovery that the laws of refractive dispersion of different substances were such as to allow of combinations which neutralized the dispersion without neutralising the refraction, is one which has hitherto been of more value to art than to science. This property has no definite bearing, which has yet been explained, upon the theory of light; but it is of the greatest importance in its application to the construction of telescopes; and it excited the more notice, in consequence of the prejudices and difficulties which for a time retarded the discovery.

Newton conceived that he had proved by experiment[1], that light is white after refraction, when the emergent rays are parallel to the incident, and in no other case. If this were so, the production of colourless images by refracting media would be impossible; and such, in deference to Newton's great authority, was for some time the general persuasion. Euler[2] observed, that a combination of lenses which does not colour the image must be possible, since we have an example of such a combination in the human eye; and

[1] Opticks, b. i. pt. ii. prop. 3.
[2] Ac. Berlin, 1747.

he investigated mathematically the conditions requisite for such a result. Klingenstierna[3], a Swedish mathematician, also showed that Newton's rule could not be universally true. Finally, John Dollond[4], in 1757, repeated Newton's experiment, and obtained an opposite result. He found that when an object was seen through two prisms, one of glass and one of water, of such angles that it did not appear displaced by refraction, it was coloured. Hence it followed that, without being coloured, the rays might be made to undergo refraction; and that thus, substituting lenses for prisms, a combination might be formed, which should produce an image without colouring it, and make the construction of an *achromatic* telescope possible.

Euler at first hesitated to confide in Dollond's experiments; but he was assured of their correctness by Clairaut, who had throughout paid great attention to the subject; and those two great mathematicians, as well as D'Alembert, proceeded to investigate mathematical formulæ which might be useful in the application of the discovery. The remainder of the deductions, which were founded upon the laws of dispersion of various refractive substances, belongs rather to the history of art than of science. Dollond used at first, for his achromatic object-glass, a lens of crown-glass, and one of flint-glass; afterwards, two lenses of the former substance, including between them one of the latter. He also adjusted the curva-

[3] Swedish Trans. 1754. [4] Phil. Trans. vol. l. 1758.

tures of his lenses in such a way as to correct the imperfections arising from the spherical form of the glasses, as well as the fault of colour. Afterwards Blair, and more recently Mr. Barlow, have used fluid media along with glass lenses, in order to produce improved object-glasses; and various mathematicians, as Sir J. Herschel and Professor Airy among ourselves, have simplified and extended the investigation of the formulæ which determine the best combinations of lenses in the object-glasses and eye-glasses of telescopes, both with reference to spherical and *chromatic* aberrations.

According to Dollond's discovery, the spectra produced by prisms of two substances, as flint-glass and crown-glass, would be of the same length when the refraction was different. But a question then occurred: When the whole distance from the red to the violet in one spectrum was the same as the whole distance in the other, were the intermediate colours, yellow, green, &c. in corresponding places in the two? This point also could not be determined any otherwise than by experiment. It appeared that such a correspondence did not exist; and, therefore, when the extreme colours were corrected by combinations of the different media, there still remained an uncorrected residue of colour arising from the rest of the spectrum. This defect was a consequence of the property, that the spectra belonging to different media were not divided in the *same ratio* by the same colours, and was hence termed the *irrationality* of the spectrum. By using three

prisms, or three lenses, three colours may be made to coincide instead of two, and the effects of this irrationality greatly diminished.

For the reasons already mentioned, we do not pursue this subject further[5], but turn to those optical facts which finally led to a great and comprehensive theory.

[5] The discovery of the *fixed lines* in the spectrum, by Wollaston and Fraunhofer, has more recently supplied the means of determining, with extreme accuracy, the corresponding portions of the spectrum in different refracting substances.

CHAPTER V.

Discovery of the Laws of Double Refraction.

The laws of refraction which we have hitherto described, were simple and uniform, and had a symmetrical reference to the surface of the refracting medium. It appeared strange to men, when their attention was drawn to a class of phenomena in which this symmetry was wanting; and in which a refraction took place which was not even in the plane of incidence. The subject was not unworthy the notice and admiration it attraction; for the prosecution of it ended in the discovery of the general laws of light. The phenomena of which I now speak, are those exhibited by various kinds of crystalline bodies; but observed for a long time in one kind only, namely, the rhombohedral calc-spar; or, as it was usually termed, from the country which supplied the largest and clearest crystals, Iceland spar. These rhombohedral crystals are usually very smooth and transparent, and often of considerable size; and it was observed, on looking through them, that all objects appeared double. The phenomena, even as early as 1669, had been considered so curious, that Erasmus Bartholin published a work upon them at Copenhagen[1], (*Experimenta Crystalli Islandici,*

[1] Priestley's Optics, p. 550.

Hafniæ, 1669.) He analysed the phenomena to their laws, so far as to discover that one of the two images was produced by refraction after the usual rule, and the other by an unusual refraction. This latter refraction Bartholin found to vary in different positions; to be regulated by a line parallel to the sides of the rhombohedron; and to be greatest in the direction of a line bisecting two of the angles of the rhombic face of the crystal.

These rules were exact as far as they went; and when we consider how geometrically complex the law is, which really regulates the unusual or extraordinary refraction,—that Newton altogether mistook it, and that it was not verified till the experiments of Haüy and Wollaston in our own time;—we might expect that it would not be soon or easily detected. But Huyghens possessed a key to the secret, in the theory, which he had devised, of the propagation of light by undulations, and which he conceived with perfect distinctness and correctness, so far as its application to these phenomena is concerned. Hence he was enabled to lay down the law of the phenomena (the only part of his discovery which we have here to consider,) with a precision and success which excited deserved admiration, when the subject, at a much later period, regained its due share of attention. His Treatise was written[1] in 1678, but not published till 1690.

The laws of ordinary and of extraordinary refrac-

[1] See his Traité de la Lumiére. Preface.

tion in Iceland spar are related to each other; they are, in fact, similar constructions, made, in the one case, by means of an imaginary sphere, in the other, by means of a spheroid; the spheroid being of such oblateness as to suit the rhombohedral form of the crystal, and the axis of the spheroid being the axis of symmetry of the crystal. Huyghens followed this general conception into particular positions and conditions; and thus obtained rules, which he compared with observation, cutting the crystal and transmitting the rays in various manners. "I have examined in detail," says he[8], "the properties of the extraordinary refraction of this crystal, to see if each phenomenon which is deduced from theory, would agree with what is really observed. And this being so, it is no slight proof of the truth of our suppositions and principles; but what I am going to add here confirms them still more wonderfully; that is, the different modes of cutting this crystal, in which the surfaces produced give rise to refractions exactly such as they ought to be, and as I had foreseen them, according to the preceding theory."

Statements of this kind, coming from a philosopher like Huyghens, were entitled to great confidence; Newton, however, appears not to have noticed, or to have disregarded them. In his "Opticks," he gives a rule for the extraordinary refraction of Iceland spar which is altogether erroneous, without assigning any reason for rejecting the

[8] See Maseres's Tracts on Optics, p. 250; or Huyghens, Tr la Lum. ch. v. Art. 43.

law published by Huyghens; and, so far as appears, without having made any experiments of his own. The Huyghenian doctrine of double refraction fell, along with his theory of undulations, into temporary neglect, of which we shall have hereafter to speak. But in 1788, Haüy showed that Huyghens's rule agreed much better than Newton's with the phenomena: and in 1802, Wollaston, applying a method of his own for measuring refraction, came to the same result. "He made," says Young[4], "a number of accurate experiments with an apparatus singularly well calculated to examine the phenomena, but could find no general principle to correct them until the work of Huyghens was pointed out to him." In 1808, the subject of double refraction was proposed as a prize-question by the French Institute; and Malus, whose Memoir obtained the prize, says, "I began by observing and measuring a long series of phenomena on natural and artificial faces of Iceland spar. Then, testing by means of these observations the different laws proposed up to the present time by physical writers, I was struck with the admirable agreement of the law of Huyghens with the phenomena, and I was soon convinced that it is really the law of nature." Pursuing the consequences of the law, he found that it satisfied phenomena which Huyghens himself had not observed. From this time, then, the truth of the Huyghenian law was universally allowed, and soon afterwards, the theory by which it had been suggested was generally received.

[4] Quart. Rev. 1809, Nov. p. 338.

The property of double refraction had been first studied only in Iceland spar, in which it is very obvious. The same property belongs, though less conspicuously, to many other kinds of crystals. Huyghens had noticed the same fact in rock-crystal[b]; and Malus found it to belong to a large list of bodies besides; for instance, arragonite, sulphate of lime, of baryta, of strontia, of iron; carbonate of lead; zircon, corundum, cymophane, emerald, euclase, felspar, mesotype, peridote, sulphur, and mellite. Attempts were made, with imperfect success, to reduce all these to the law which had been established for Iceland spar. In the first instance, Malus took for granted that the extraordinary refraction depended always upon an oblate spheroid; but Biot[c] pointed out a distinction between two classes of crystals in which this spheroid was oblong and oblate respectively, and these he called *attractive* and *repulsive* crystals. With this correction, the law could be extended to a considerable number of cases; but it was afterwards perceived, that even in this form, it belonged only to substances of which the crystallization has relation to a single axis of symmetry, as the rhombohedron, or the square pyramid. In other cases, as the rhombic prism, in which the form, considered with reference to its crystalline symmetry, is *biaxal*, the law is much more complicated. In that case, the sphere and spheroid, which are used in the construction for

[b] Tr. de la Lum. Ch. v. Art. 20.

[c] Biot, Traité de Phys. iii. 330.

uniaxal crystals, transform themselves into the two successive convolutions of a single continuous curve surface; neither of the two rays follows the law of ordinary refraction; and the formula which determines their position is very complex. It is, however, capable of being tested by measures of the refraction of crystals cut in a peculiar manner for the purpose, and this was done by Fresnel and Arago. But this complex law of double refraction was only discovered through the aid of the theory of a luminiferous ether, and therefore we must now return to the other facts which led to such a theory.

CHAPTER VI.

Discovery of the Laws of Polarization.

If the Extraordinary Refraction of Iceland spar had appeared strange, another phenomenon was soon noticed in the same substance, which appeared stranger still, and which in the sequel was found to be no less important. I speak of the facts which were afterwards described under the term *Polarization*. Huyghens was the discoverer of this class of facts. At the end of the treatise which we have already quoted, he says[1], "Before I quit the subject of this crystal, I will add one other marvellous phenomenon, which I have discovered since writing the above; for though hitherto I have not been able to find out its cause, I will not, on that account, omit pointing it out, that I may give occasion to others to examine it." He then states the phenomena; which are, that when two rhombohedrons of Iceland spar are in parallel positions, a ray doubly refracted by the first, is not further divided when it falls on the second: the ordinarily refracted ray is ordinarily refracted *only*, and the extraordinary ray is only extraordinarily refracted by the second crystal, neither ray being doubly refracted. The same is still the case, if the two crystals have their *principal*

[1] Tr. Opt. p. 252.

planes parallel, though they themselves are not parallel. But if the principal plane of the second crystal be perpendicular to that of the first, the reverse of what has been described takes place; the ordinarily refracted ray of the first crystal suffers, at the second, extraordinary refraction *only*, and the extraordinary ray of the first suffers ordinary refraction only at the second. Thus, in each of these positions, the double refraction of each ray at the second crystal is reduced to single refraction, though in a different manner in the two cases. But in any other position of the crystals, each ray, produced by the first, is doubly refracted by the second, so as to produce four rays.

A step in the right conception of these phenomena was made by Newton, in the second edition of his Opticks (1717). He represented them as resulting from this;—that the rays of light have "sides," and that they undergo the ordinary or extraordinary refraction, according as these sides are parallel to the principal plane of the crystal, or at right angles to it (Query 26). In this way, it is clear, that those rays which, in the first crystal, had been selected for extraordinary refraction, because their sides were perpendicular to the principal plane, would all suffer extraordinary refraction at the second crystal for the same reason, if its principal plane were parallel to that of the first; and would all suffer ordinary refraction, if the principal plane of the second crystal were perpendicular to that of the first, and consequently parallel to the sides of the refracted

ray. This view of the subject includes some of the leading features of the case, but still leaves several considerable difficulties.

No material advance was made in the subject till it was taken up by Malus[1], along with the other circumstances of double refraction, about a hundred years afterwards. He verified what had been observed by Huyghens and Newton, on the subject of the variations which light thus exhibits; but he discovered that this modification, in virtue of which light undergoes the ordinary, or the extraordinary, refraction, according to the position of the plane of the crystal, may be impressed upon it in many other ways. One part of this discovery was made accidentally[2]. In 1808, Malus happened to be observing the light of the setting sun, reflected from the windows of the Luxembourg, through a rhombohedron of Iceland spar; and he observed that in turning round the crystal, the two images varied in their intensity. Neither of the images completely vanished, because the light from the windows was not properly modified, or, to use the term which Malus soon adopted, was not completely *polarized*. The complete polarization of light by reflection from glass, or any other transparent substance, was found to take place at a certain definite angle, different for each substance. It was found also that in all crystals in which double refraction occurred, it was

[1] Malus, Th. de la Doub. Ref. p. 296.

[2] Arago, art. Polarization, Supp. Enc. Brit.

accompanied by polarization; the two rays, the ordinary and the extraordinary, being always polarized *oppositely*, that is, in planes at right angles to each other. It was further found, that the modification of light so produced, or the nature of the polarization, was identical in all these cases;—that the alternatives of ordinary and extraordinary refraction and non-refraction, were the same, by whatever crystal they were tested, or in whatever manner the polarization had been impressed upon the light; in short, that the property, when once acquired, was independent of everything except the sides or *poles* of the ray; and thus, in 1811, the term "polarization" was introduced[4].

This being the state of the subject, it became an obvious question, by what other means, and by what laws, this property was communicated. It was found that some crystals, instead of giving, by double refraction, two images oppositely polarized, give a single polarized image. This property was discovered in tourmaline by Seebeck in 1813, and by Biot in 1814; and that mineral became, in consequence, a very convenient part of the apparatus used in such observations. Various peculiarities bearing upon this subject, were detected by different experimenters. It was in a short time discovered, that light might be polarized by refraction, as well as by reflection, at the surface of uncrystallized bodies, as glass; the plane of polarization being the plane of

[4] Mém. Inst. 1810.

reflection; further, that when a portion of a ray of light was polarized by reflection, a corresponding portion was polarized by transmission, the planes of the two polarizations being at right angles to each other. It was found also that the polarization which was incomplete with a single plate, either by reflection or refraction, might be made more and more complete by increasing the number of plates.

Among an accumulation of phenomena like this, it is our business to inquire what general laws were discovered. To make such discoveries without possessing the general theory of the facts, required no ordinary sagacity and good fortune. Yet several laws were detected at this stage of the subject. Malus, in 1811, obtained the important generalization that, whenever we obtain, by any means, a polarized ray of light, we produce also another ray, polarized in a contrary direction; thus when reflection gives a polarized ray, the companion-ray is refracted polarized oppositely, along with a quantity also of unpolarized light. And we must particularly notice *Sir D. Brewster's rule* for the *polarizing angle* of different bodies.

Malus[5] had said that the angle of reflection from transparent bodies, which most completely polarizes the reflected ray, does not follow any discoverable rule with regard to the order of refractive or dispersive powers of the substances. Yet the rule was in reality very simple. In 1815, Brewster stated[6]

[5] Mém. Inst. 1810. [6] Ph. Tr. 1815.

as the law, which in all cases determines this angle, that "the index of refraction is the tangent of the angle of polarization." It follows from this, that the polarization takes place when the reflected and refracted rays are at right angles to each other. This simple and elegant rule has been fully confirmed by all subsequent observations, as by those of Biot and by Seebeck; and must be considered one of the happiest and most important discoveries of the laws of phenomena in Optics.

The rule for polarization by one reflection being thus discovered, tentative formulæ were proposed by Brewster and Biot, for the cases in which several reflections or refractions take place. Fresnel also, in 1817 and 1818, traced the effect of reflection in modifying the direction of polarization, which Malus had done inaccurately in 1810. But the complexity of the subject made all such attempts extremely precarious, till the theory of the phenomena was understood, a period which now comes under notice. The laws which we have spoken of were important materials for the establishment of the theory; but in the mean time, its progress at first had been more forwarded by some other classes of facts, of a different kind, and of a longer standing notoriety, to which we must now turn our attention.

CHAPTER VII.

Discovery of the Laws of the Colours of Thin Plates.

The facts which we have now to consider are remarkable, inasmuch as the colours are produced merely by the smallness of dimensions of the bodies employed. The light is not analysed by any peculiar property of the substances, but dissected by the minuteness of their parts. On this account, these phenomena give very important indications of the real structure of light; and at an early period, suggested views which are, in a great measure, just.

Hooke appears to be the first person who made any progress in discovering the laws of the colours of thin plates. In his "Micrographia," printed by the Royal Society in 1664, he describes, in a detailed and systematic manner, several phenomena of this kind, which he calls "fantastical colours." He examined them in Muscovy glass or mica, a transparent mineral which is capable of being split into the exceedingly thin films which are requisite for such colours; he noticed them also in the fissures of the same substance, in bubbles blown of water, rosin, gum, glass; in the films on the surface of tempered steel; between two plane pieces of glass; and in other cases. He perceived also[1], that the

[1] Micrographia, p. 53.

production of each colour required a plate of determinate thickness, and he employed this circumstance as one of the grounds of his theory of light.

Newton took up the subject where Hooke had left it; and followed it out with his accustomed skill and clearness, in his "Discourse on Light and Colours," communicated to the Royal Society in 1675. He determined, what Hooke had not ascertained, the thickness of the film which was requisite for the production of each colour; and in this way explained, in a complete and admirable manner, the coloured rings which occur when two lenses are pressed together, and the *scale of colour* which the rings follow; a step of the more consequence, as the same scale occurs in many other optical phenomena.

It is not our business here to state the hypothesis with regard to the properties of light which Newton founded on these facts;—the "fits of easy transmission and reflection." We shall see hereafter that his attempted induction was imperfect; and his endeavour to account, by means of the laws of thin plates, for the colours of natural bodies, is altogether unsatisfactory. But notwithstanding these failures in his speculations on this subject, he did make in it some very important steps; for he clearly ascertained that when the thickness of the plate was about 1-178000th of an inch, or three times, five times, seven times that magnitude, there was a bright colour produced; but blackness, when the thickness was exactly intermediate between those magnitudes. He found, also, that the thicknesses which gave red

and violet[9] were as fourteen to nine; and the intermediate colours of course corresponded to intermediate thicknesses, and therefore, in his apparatus, consisting of two lenses pressed together, gave rings of intermediate sizes. His mode of confirming the rule, by throwing upon this apparatus differently coloured homogeneous light, is striking and elegant. "It was very pleasant," he says, "to see the rings gradually swell and contract as the colour of the light was changed."

It is not necessary to enter further into the detail of these phenomena, or to notice the rings seen by transmission, and other circumstances. The important step made by Newton in this matter was, the showing that the rays of light, in these experiments, go periodically through certain cycles of modification, each period occupying nearly the small fraction of an inch mentioned above; and this interval being different for different colours. Although Newton did not correctly disentangle the conditions under which this periodical character is manifestly disclosed, the discovery that, under some circumstances, it does exist, was likely to influence, and did influence, materially and beneficially, the subsequent progress of optics towards a connected theory.

We must now trace this progress; but before we proceed to this task, we will briefly notice a number of optical phenomena which had been collected, and which waited for the touch of sound theory to introduce among them that rule and order which mere observation had sought for in vain.

[9] Opticks, p. 184.

CHAPTER VIII.

Attempts to Discover the Laws of other Phenomena.

The phenomena which result from optical combinations, even of a comparatively simple nature, are extremely complex. The theory accounts for these results with the most curious exactness, and points out the laws which pervade the apparent confusion; but without this key to the appearances, it was scarcely possible that any rule or order should be detected. The undertaking was of the same kind as it would have been, to discover all the inequalities of the moon's motion without the aid of the doctrine of gravity. We will enumerate some of the phenomena which thus employed and perplexed the cultivators of optics.

The fringes of shadows were one of the most curious and noted of such classes of facts. These were first remarked by Grimaldi[1], (1665,) and referred by him to a property of light which he called *Diffraction*. When shadows are made in a dark room, by light admitted through a very small hole, these appearances are very conspicuous and beautiful. Hooke, in 1672, communicated similar observations to the Royal Society, as "a new property of

[1] Physico-Mathesis, de Lumine, Coloribus et Iride. Bologna, 1665.

light not mentioned by any optical writer before;" by which we see that he had not heard of Grimaldi's experiments. Newton, in his Opticks, treats of the same phenomena, which he ascribes to the *inflexion* of the rays of light. He asks (Qu. 3,) "Are not the rays of light, in passing by the edges and sides of bodies, bent several times backward and forward with a motion like that of an eel? And do not the three fringes of coloured light in shadows arise from three such bendings?" It is remarkable that Newton should not have noticed, that it is impossible, in this way, to account for the facts, or even to express their laws; since the light which produces the fringes is propagated, even after it leaves the neighbourhood of the opake body, in curves, and not in straight lines. Accordingly, all who have taken up Newton's notion of inflexion, have inevitably failed in giving anything like an intelligible and coherent character to these phenomena. This is, for example, the case with Mr. (now Lord) Brougham's attempts in the Philosophical Transactions for 1796. The same may be said of other experimenters, as Mairan[2] and Du Four[3], who attempted to explain the facts by supposing an atmosphere about the opake body. Several authors, as Maraldi[4], and Comparetti[5], repeated or varied these experiments in different ways.

Newton had noticed certain rings of colour pro-

[2] Ac. Par. 1738.

[3] Mémoires Presentés, vol. v.

[4] Ac. Par. 1723.

[5] Observationes Opticæ de Luce Inflexâ et Coloribus. Padua, 1787.

duced by a glass speculum, which he called "colours of thick plates," and which he attempted to connect with the colours of thin plates. His reasoning is by no means satisfactory; but it was of use, by pointing out this as a case in which his "fits" (the small periods, or cycles in the rays of light, of which we have spoken,) continued to occur for a considerable length of the ray. But other persons, attempting to repeat his experiments, confounded with them extraneous phenomena of other kinds; as the Duc de Chaulnes, who spread muslin before his mirror[6], and Dr. Herschel, who scattered hair-powder before his[7]. The colours produced by the muslin were those belonging to shadows of *gratings*, afterwards examined more successfully by Frauenhofer, when in possession of the theory. We may mention here also the colours which appear on finely striated surfaces, and on mother-of-pearl, feathers, and similar substances. These had been examined by various persons, (as Boyle, Mazeas, Brougham,) but could still, at this period, be only looked upon as insulated and lawless facts.

[6] A. P. 1755. [7] P. T. 1807.

CHAPTER IX.

Discovery of the Laws of Phenomena of Dipolarized Light.

Besides the above-mentioned perplexing cases of colours produced by common light, cases of *periodical colours produced by polarized light* began to be discovered, and soon became numerous. In August 1811, M. Arago communicated to the Institute of France an account of colours seen by passing polarized light through mica, and analysing it with a prism of Iceland spar. It is remarkable that the light which produced the colours in this case was the light polarized by the sky, a cause of polarization not previously known. The effect which the mica thus produced was termed *depolarization*;—not a very happy term, since the effect is not the destruction of the polarization, but the combination of a new polarizing influence with the former. The word *dipolarization*, which has since been proposed, is a much more appropriate expression. Several other curious phenomena of the same kind were observed in quartz, and in flint-glass. Arago was not able to reduce these phenomena to laws, but he had a full conviction of their value, and ventures to class them with the great steps in this part of optics. "To Bartholin we owe the knowledge of double refraction; to Huyghens, that of the accompanying

polarization; to Malus, polarization by reflection; to Arago, depolarization." Brewster was at the same time engaged in a similar train of research; and made discoveries of the same nature, which, though not published till some time after those of Arago, were obtained without a knowledge of what had been done by him. Brewster's "Treatise on New Philosophical Instruments," published in 1813, contains many curious experiments on the "depolarizing" properties of minerals. Both these observers noticed the changes of colour which are produced by changes in the position of the ray, and the alternations of colour in the two oppositely polarized images; and Brewster discovered that, in topaz, the phenomena had a certain reference to lines which he called the *neutral* and *depolarizing* axes. Biot had endeavoured to reduce the phenomena to a law; and had succeeded so far, that he found that in the plates of sulphate of lime, the place of the tint, estimated in Newton's scale, was as the square of the sine of the inclination. But the laws of these phenomena became much more obvious when they were observed by Brewster with a larger field of view[1]. He found that the colours of topaz, under the circumstances now described, exhibited themselves in the form of elliptical rings, crossed by a black bar, "the most brilliant class of phenomena," as he justly says, "in the whole range of optics." In 1814, also, Wollaston observed the circular rings with a black

[1] Phil. Tr. 1814.

cross, produced by similar means in calc-spar; and Biot, in 1815, made the same observation. The rings, in several of these cases, were carefully measured by Biot and Brewster, and a great mass of similar phenomena was discovered. These were added to by various persons, as Seebeck, and Herschel the younger.

The question of priority of discovery respecting these facts and their laws excited some national jealousy. The following is the manner in which M. Arago, writing anonymously, spoke on this subject[2]: "Dr. Brewster, in publishing his experiments in 1813, says that they were made before he had seen M. Arago's paper, 'and even before any of his countrymen had any knowledge of what had been done in France.' (Edinburgh Encyclopædia, art. Optics, p. 587.) We must take Dr. Brewster's word for the first part of this assertion; but since an extract of M. Arago's Memoir was inserted in the 'Moniteur' on the 30th August, 1811, there would be some difficulty in *proving* the truth of the second part."

And M. Biot complains of Dr. Brewster's Memoir of 1818 as not drawn up in the principles of mutual indulgence[3]. He allows to the British philosopher that he explained how the deviations of the colours from Newton's scale are produced by the influence of two axes[4]; and that he gave for the coloured curves a law, empirical indeed, but very

[2] Supp. Enc. Brit., art. Polarisation of Light.

[3] *Mem. Ins.* 1818, p. 180. [4] Ib. p. 191.

faithfully predicting the varieties of their forms[5]; but he justly claims for himself the merit of having given the first formulæ which represented the apparently anomalous succession of colours in a biaxal crystal, namely, Siberian mica[6].

Brewster, in 1818, discovered a general relation between the crystalline form and the optical properties, which gave an incalculable impulse and a new clearness to these researches. He found that there was a correspondence between the degree of symmetry of the optical phenomena and the crystalline form; those crystals which are uniaxal in the crytallographical sense, are also uniaxal in their optical properties, and give circular rings; those which are of other forms are, generally speaking, biaxal; they give oval and knotted *isochromatic* lines, with two *poles*. He also discovered a law for the tint at each point in such cases. This law, when simplified by Biot[7], made the tint proportional to the product of the distances of the point from the two poles. In the following year, Herschel confirmed this law by showing, from actual measurement, that the curve of the isochromatic lines in these cases was the curve termed the *lemniscata*, which has, for each point, the product of the distances from two fixed poles equal to a constant quantity[8]. He also reduced to rule some other apparent anomalies in phenomena of the same class.

[5] Mém. Inst. 1818, p. 196.
[6] Ib. p. 192.
[7] Ib.
[8] Phil. Trans. 1819.

Biot, too, gave a rule for the directions of the planes of polarization of the two rays produced by double refraction in biaxal crystals, a circumstance which has a close bearing upon the phenomena of dipolarization. His rule was, that the one plane of polarization bisects the dihedral angle formed by the two planes which pass through the optic axes, and that the other is perpendicular to such a plane. When, however, Fresnel had discovered from the theory the true laws of double refraction, it appeared that the above rule is inaccurate, although in a degree which observation could hardly detect without the aid of theory[9].

There were still other classes of optical phenomena which attracted notice; especially those which are exhibited by plates of quartz cut perpendicular to the axis. Arago had observed, in 1811, that this substance produced a *twist*, to the right or left hand, in the plane of polarization; a result which was afterwards traced to a modification of light different both from common and from polarized light, and known as *circular polarization;* and in 1815, Biot[10] found that several fluids possessed the same curious property. Herschel had the good fortune to discover that this peculiar kind of polarization in quartz was connected with an equally peculiar modification of crystallization, the *plagihedral* faces which are seen, on some crystals, obliquely disposed,

[9] Fresnel, Mém. Inst. 1827, p. 162.
Traité de Phys. iv. 542.

and, as it were, following each other round the crystal from left to right, or from right to left. Herschel found that the *right-handed* or *left-handed* character of the circular polarization corresponded, in all cases, to that of the crystal.

It will easily be supposed that all these brilliant phenomena could not be observed, and the laws of many of the phenomena discovered, without attempts on the part of philosophers to combine them all under the dominion of some wide and profound theory. Endeavours to ascend from such knowledge as we have spoken of, to the general theory of light, were, in fact, made at every stage of the subject, and with a success which at last won almost all suffrages. We are now arrived at the point at which we are called upon to trace the history of this theory; to pass from the laws of phenomena to their causes;—from Formal to Physical Optics. The undulatory theory of light, the only discovery which can stand by the side of the theory of universal gravitation, as a doctrine belonging to the same order, for its generality, its fertility, and its certainty, may properly be treated of with that ceremony which we have hitherto bestowed only on the great advances of astronomy; and I shall therefore now proceed to speak of the Prelude to this epoch, the Epoch itself, and its Sequel, according to the form of the preceding book.

PHYSICAL OPTICS.

CHAPTER X.

Prelude to the Epoch of Young and Fresnel.

By Physical Optics we mean, as has already been stated, the theories which explain optical phenomena on mechanical principles. No such explanation could be given till true mechanical principles had been obtained; and, accordingly, we must date the commencement of the essays towards physical optics from Descartes, the founder of the modern mechanical philosophy. His hypothesis concerning light is, that it consists of small particles emitted by the luminous body. He compares these particles to balls, and endeavours to explain, by means of this comparison, the laws of reflection and refraction[1]. In order to account for the production of colours by refraction, he ascribes to these balls an alternating rotatory motion[2]. This first form of the *emission theory*, was, like most of the physical speculations of its author, hasty and gratuitous; but was extensively accepted, like the rest of the Cartesian doctrines, in consequence of the love which men have for sweeping and simple dogmas, and deductive reasonings from them. In a short time, however, the rival

[1] Diopt. c. ii. 4. [2] Meteor

optical *theory of undulations* made its appearance. Hooke in his Micrographia (1664) propounds it, upon occasion of his observations, already noticed, on the colours of thin plates. He there asserts[3] light to consist in a "quick, short, vibrating motion," and that it is propagated in a homogeneous medium, in such a way that "every pulse or vibration of the luminous body will generate a sphere, which will continually increase and grow bigger, just after the same manner (though indefinitely swifter) as the waves or rings on the surface of water do swell into bigger and bigger circles about a point in it[4]." He applies this to the explanation of refraction, by supposing that the rays in a denser medium move more easily, and hence that the pulses become oblique, a far less satisfactory and consistent hypothesis than that of Huyghens. But Hooke has the merit of having also combined with his theory, though somewhat obscurely, the *principle* of *interferences*, in the application which he makes of it to the colours of thin plates. Thus[5] he supposes the light to be reflected at the first surface of such plates; and he adds, "after two refractions and one reflection (from the second surface) there is progagated a kind of fainter ray," which comes behind the other reflected pulse; "so that hereby (the surfaces A B and E F being so near together that the eye cannot discriminate them from one,) this compound or duplicated pulse does produce on the retina the sensation of a yellow." The reason for the production of this particular

[3] Micrographia, p. 56. [4] Ib. p. 57. [5] Ib. p. 66.

colour, in the case of which he here speaks, depends on his views concerning the kind of pulses appropriate to each colour; and, for the same reason, when the thickness is different, he finds that the result will be a red or a green. This is a very remarkable anticipation of the explanation ultimately given of these colours; and we may observe, that if Hooke could have measured the thickness of his thin plates, he could hardly have avoided making considerable progress in the doctrine of interferences.

But the person who is generally, and with justice, looked upon as the great author of the undulatory theory at the period now under notice, is Huyghens, whose *Traité de la Lumiere*, containing a developement of his theory, was written in 1678, though not published till 1690. In this work he maintained, as Hooke had done, that light consists in undulations, and expands itself spherically, nearly in the same manner as sound; and he referred to the observations of Römer on Jupiter's satellites, both to prove that this difference takes place successively, and to show its exceeding swiftness. In order to trace the effect of an undulation, Huyghens considers that every point of a wave diffuses its motion in all directions; and hence he draws the conclusion, so long looked upon as the turning-point of the combat between the rival theories, that the light will not be diffused beyond the rectilinear space, when it passes through an aperture; "for," says he[6], "although the

[6] Tracts on Optics, p. 209.

partial waves, produced by the particles comprised in the aperture, do diffuse themselves beyond the rectilinear space, these waves do not *concur* anywhere except in front of the aperture." He rightly considers this observation as of the most essential value. "This," he says, "was not known by those who began to consider the waves of light, among whom are Mr. Hooke in his Micrography, and Father Pardies, who, in a treatise of which he showed me a part, and which he did not live to finish, had undertaken to prove, by these waves, the effects of reflection and refraction. But the principal foundation, which consists in the remark I have just made, was wanting in his demonstrations."

By the help of this view, Huyghens gave a perfectly satisfactory and correct explanation of the laws of reflection and refraction; and he also applied the same theory, as we have seen, to the double refraction of Iceland spar with great sagacity and success. He conceived that in this crystal, besides the spherical waves, there might be others of a spheroidal form, the axis of the spheroid being symmetrically disposed with regard to the faces of the rhombohedron, for to these faces the optical phenomena are symmetrically related. He found[7] that the position of the refracted ray, determined by such spheroidal undulations, would give an oblique refraction, which would coincide in its laws with the refraction observed in Iceland spar: and, as we have

[7] Tracts on Optics, p. 237.

stated, this coincidence was long after fully confirmed by other observers.

Since Huyghens, at this early period, expounded the undulatory theory with so much distinctness, and applied it with so much skill, it may be asked why we do not hold him up as the great author of the induction of undulations of light;—the person who marks the epoch of the theory? To this we reply, that though Huyghens discovered strong presumptions in favour of the undulatory theory, it was not *established* till a later era, when the fringes of shadows, rightly understood, made the waves visible, and when the hypothesis which had been assumed to account for double refraction, was found to contain also an explanation of polarization. It is *then* that this theory of light assumes its commanding form; and the persons who gave it this form, we must make the great names of our narrative; without, however, denying the genius and merit of Huyghens, who is, undoubtedly, the leading character in the prelude to the discovery.

The undulatory theory, from this time to our own, was unfortunate in its career. It was by no means destitute of defenders, but these were not experimenters; and none of them thought of applying it to Grimaldi's experiments on fringes, of which we have spoken a little while ago. And the great authority of the period, Newton, adopted the opposite hypothesis, that of emission, and gave it a currency among his followers which kept down the sounder theory for above a century.

Newton's first disposition appears to have been by no means averse to the assumption of an ether as the vehicle of luminiferous undulations. When Hooke brought against his prismatic analysis of light some objections, founded on his own hypothetical notions, Newton, in his reply, said[8], "The hypothesis has a much greater affinity with his own hypothesis than he seems to be aware of; the vibrations of the ether being as useful and necessary in this as in his." This was in 1672; and we might produce, from Newton's writings, passages of the same kind, of a much later date. Indeed it would seem that, to the last, Newton considered the assumption of an ether as highly probable, and its vibrations as important parts of the phenomena of light; but he also introduced into his system the hypothesis of emission, and having followed this hypothesis into mathematical detail, while he has left all that concerns the ether in the form of queries and conjectures, the emission theory has naturally been treated as the leading part of his optical doctrines.

The principal propositions of the Principia which bear upon the question of optical theory are those of the fourteenth Section of the first Book[9], in which the law of the sines in refraction is proved on the hypothesis that the particles of bodies act on light only at very small distances; and the proposition of the eighth Section of the second Book[10], in which it

[8] Ph. Tr. vii. 5087.

[9] Principia, Prop. 94, et seq.

[10] Ib. Prop. 42.

is pretended to be demonstrated that the motion propagated in a fluid must diverge when it has passed through an aperture. The former proposition shows that the law of refraction, an optical truth which mainly affected the choice of a theory, (for about reflection there is no difficulty on any mechanical hypothesis,) follows from the theory of emission: the latter proposition is conceived to prove the inadmissibility of the rival hypothesis, that of undulations. As to the former point,—the hypothetical explanation of refraction, on the assumptions there made,—the conclusion is quite satisfactory: but the reasoning in the latter case, (respecting the propagation of undulations,) is certainly inconclusive and vague; and something better might the more reasonably have been expected, since Huyghens had at least endeavoured to prove the opposite proposition. But supposing we leave these properties, the rectilinear course, the reflection, and the refraction of light, as problems in which neither theory has a decided advantage, what is the next material point? The colours of thin plates. Now, how does Newton's theory explain these? By a new and special supposition;—that of *fits of easy transmission and reflection:* a supposition which, though it truly expresses the facts, is not borne out by any other phenomena. But, passing over this, when we come to the peculiar laws of polarization in Iceland spar, how does Newton's meet these? Again by a special and new supposition;—that the rays of light have *sides.* Thus we find no fresh evidence in favour of the

emission-hypothesis springing out of the fresh demands made upon it. It may be urged, in reply, that the same is true of the undulatory theory; and it must be allowed that, at the time of which we now speak, its superiority in this respect was not manifested; though Hooke, as we have seen, had caught a glimpse of the explanation, which this theory supplies, of the colours of thin plates.

At a later period, Newton certainly seems to have been strongly disinclined to believe light to consist in undulations merely. "Are not," he says, in Question twenty-eight of the Opticks, "all hypotheses erroneous, in which light is supposed to consist in pression or motion propagated through a fluid medium?" The arguments which most weighed with him to produce this conviction, appear to have been the one already mentioned,—that, on the undulatory hypothesis, undulations passing through an aperture would be diffused; and again,—his conviction, that the properties of light, developed in various optical phenomena, "depend not upon new modifications, but upon the original and unchangeable properties of the rays." (Question twenty-seven.)

But yet, even in this state of his views, he was very far from abandoning the machinery of vibrations altogether. He is disposed to use such machinery to produce his "fits of easy transmission." In his seventeenth Query, he says[11], "when a ray of light falls upon the surface of any pellucid body, and is

[11] Opticks, p. 322.

there refracted or reflected; may not waves of vibrations or tremors be thereby excited in the refracting or reflecting medium at the point of incidence? and do not these vibrations overtake the rays of light, and by overtaking them successively, do they not put them into the fits of easy reflection and easy transmission described above?" Several of the other queries imply the same persuasion, of the necessity for the assumption of an ether and its vibrations. And it might have been asked, whether any good reason could be given for the hypothesis of an ether as a *part* of the mechanism of light, which would not be equally valid in favour of this being the *whole* of the mechanism, especially if it could be shown that nothing more was wanted to produce the results.

The emission theory was, however, embraced in the most strenuous manner by the disciples of Newton. That propositions existed in the Principia which proceeded on this hypothesis, was, with many of these persons, ground enough for adopting the doctrine; and it had also the advantage of being more ready of conception, for though the propagation of a wave is not very difficult to conceive, at least by a mathematician, the motion of a particle is still easier.

On the other hand, the undulation theory was maintained by no less a person than Euler; and the war between the two opinions was carried on with great earnestness. The arguments on one side and on the other soon became trite and familiar, for no

person explained any new class of facts by either theory. Thus it was urged by Euler against the system of emission[18],—that the perpetual emanation of light from the sun must have diminished his mass;—that the stream of matter thus constantly flowing must affect the motions of the planets and comets;—that the rays must disturb each other;—that the passage of light through transparent bodies is, on this system, inconceivable: all such arguments were answered by representations of the exceeding minuteness and velocity of the matter of light. On the other hand, there was urged against the theory of waves, the favourite Newtonian argument, that on this theory the light passing through an aperture ought to be diffused, as sound is. It is curious that Euler does not make to this argument the reply which Huyghens had made before. The fact really was, that he was not aware of the true ground of the difference of the result in the cases of sound and light; namely, that any ordinary aperture bears an immense ratio to the length of an undulation of light, but does not bear a very great ratio to the length of an undulation of sound. The demonstrable consequence of this difference is, that light darts through such an orifice in straight rays, while sound is diffused in all directions. Euler, not perceiving this difference, rested his answer mainly upon a circumstance by no means unimportant, that the partitions usually employed are not impermeable

[18] Fischer, iv. 449.

to sound, as opake bodies are to light. He observes that the sound does not all come through the aperture; for we hear, though the aperture be stopped. These were the main original points of attack and defence, and they continued nearly the same for the whole of the last century; the same difficulties were over and over again proposed, and the same solutions given, much in the manner of the disputations of the schoolmen of the middle ages.

The struggle being thus apparently balanced, the scale was naturally turned by the general ascendancy of the Newtonian doctrines; and the emission theory was the one most generally adopted. It was still more firmly established, in consequence of the turn generally taken by the scientific activity of the latter half of the eighteenth century; for while nothing was added to our knowledge of optical laws, the chemical effects of light were studied to a considerable extent by various inquirers[18]; and the opinions at which these persons arrived, they found that they could express most readily, in consistency with the reigning chemical views, by assuming the materiality of light. It is, however, clear, that no reasonings of the inevitably vague and doubtful character which belong to these portions of chemistry, ought to be allowed to interfere with the steady and regular

[18] As Scheele, Selle, Lavoisier, De Luc, Richter, Leonhardi, Gren, Girtanner, Link, Hagen, Voigt, De la Metherie, Scherer, Dizé, Brugnatelli. See Fischer, vii. p. 20.

progress of induction and generalisation, founded on relations of space and number, by which procedure the mechanical sciences are formed. We reject, therefore, all these chemical speculations, as belonging to other subjects; and consider the history of optical theory as a blank, till we arrive at some very different events, of which we have now to speak.

CHAPTER XI.

Epoch of Young and Fresnel.

Sect. 1.—*Introduction.*

The man whose name must occupy the most distinguished place in the history of Physical Optics, in consequence of what he did in reviving and establishing the undulatory theory of light, is Dr. Thomas Young. He was born in 1773, at Milverton in Somersetshire, of Quaker parents; and after distinguishing himself during youth, by the variety and accuracy of his attainments, he settled in London as a physician in 1801; but continued to give much of his attention to general science. His optical theory, for a long time, made few proselytes; and several years afterwards, Auguste Fresnel, an eminent French "ingenieur" and mathematician, took up similar views, proved their truth, and traced their consequences, by a series of labours almost independent of those of Dr. Young. It was not till the theory was thus re-echoed from another land, that it was able to take any strong hold on the attention of the countrymen of its earlier promulgator.

The theory of undulations, like that of universal gravitation, may be divided into several successive steps of generalisation. In both cases, all these steps were made by the same persons; but there is

this difference;—all the parts of the law of universal gravitation were worked out in one burst of inspiration by its author, and published at one time;—in the doctrine of light, on the other hand, the different steps of the advance were made and published at separate times, with intervals between. We see the theory in a narrower form, and in detached portions, before the widest generalisations and principles of unity are reached; we see the authors struggling with the difficulties before we see them successful. They appear to us as men like ourselves, liable to perplexity and failure, instead of coming before us, as Newton does in the history of Physical Astronomy, as an irresistible and almost supernatural hero of a philosophical romance.

The main subdivisions of the great advance in physical optics, of which we have now to give an account, are the following:—

1. The explanation of the *periodical colours* of thin plates, thick plates, fringed shadows, striated surfaces, and other phenomena of the same kind, by means of the doctrine of the *interference* of undulations.

2. The explanation of the phenomena of *double refraction* by the propagation of undulations in a medium of which the optical *elasticity* is different in different directions.

3. The conception of *polarization* as the result of the vibrations being *transverse;* and the consequent explanation of the production of polarization, and

the necessary connexion between polarization and double refraction, on mechanical principles.

4. The explanation of the phenomena of *dipolarization*, by means of the interference of the *resolved parts* of the vibrations after double refraction.

The history of each of these discoveries will be given separately to a certain extent; by which means the force of proof arising from their combination will be more apparent.

Sect. 2.—Explanation of the Periodical Colours of Thin Plates and Shadows by the Undulatory Theory.

THE explanation of periodical colours by the principle of interference of vibrations, was the first step which Young made in his confirmation of the undulatory theory. In a paper on Sound and Light, dated Emanuel College, Cambridge, 8th July, 1799, and read at the Royal Society in January following, he appears to incline strongly to the Huyghenian theory; not however offering any new facts or calculations in its favour, but pointing out the great difficulties of the Newtonian hypothesis. But in a paper read before the Royal Society, November 12, 1801, he says, "A further consideration of the colours of *thin plates* has converted that prepossession which I before entertained for the undulatory theory of light, into a very strong conviction of its truth and efficiency; a conviction which has since been most

strikingly confirmed by an analysis of the colours of *striated surfaces.*" He here states the general principle of interferences in the form of a proposition. (Prop. viii.) "When two undulations from different origins coincide either perfectly or very nearly in direction, their joint effect is a combination of the motions belonging to them." He explains, by the help of this proposition, the colours which were observed in Coventry's micrometers, in which instrument lines were drawn on glass at the distance of 1-500th of an inch. The interference of the undulations of the rays reflected from the two sides of these fine lines, produced periodical colours. In the same manner, he accounts for the colours of thin plates, by the interference of the light partially reflected from the two surfaces. We have already seen that Hooke had long before suggested the same explanation; and Young says at the end of his paper, "It was not till I had satisfied myself respecting all these phenomena, that I found in Hooke's Micrographia a passage which might have led me earlier to a similar opinion." He also quotes from Newton many passages which assume the existence of an ether; of which, as we have already seen, Newton suggests the necessity in these very phenomena, though he would apply it in combination with the emission of material light. In July, 1802, Young explained, on the same principle, some facts in indistinct vision, and other similar appearances. And in 1803[1], he speaks more positively still. "In

[1] Phil. Trans. Memoir, read Nov. 24,

making," he says, "some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society a short statement of the facts which appear to me to be thus decisive." The two papers just mentioned certainly ought to have convinced all scientific men of the truth of the doctrine thus urged; for the number and exactness of the explanations is very remarkable. They include the coloured fringes which are seen with fibres; the colours produced by a dew between two pieces of glass, which, according to the theory, should appear when the thickness is *six* times that of thin plates, and which do so; the changes resulting from the employment of other fluids than water; the effect of inclining the plates; also the fringes and bands which accompany shadows, the phenomena observed by Grimaldi, Newton, Maraldi, and others, and hitherto never at all reduced to rule. Young observes, very justly, "whatever may be thought of the theory, we have got a simple and general law" of the phenomena. He moreover calculated the length of an undulation from the measurements of fringes of shadows, as he had done before from the colours of thin plates; and found a very close accordance of the results of the various cases with one another.

There is one difficulty, and one inaccuracy, in Young's views at this period, which it may be proper

to note. The difficulty was, that he found it necessary to suppose that light, when reflected at a rarer medium, is retarded by half an undulation. This assumption, though often urged at a later period as an argument against the theory, was fully justified as the mechanical principles of the subject were unfolded; and the necessity of it was clear to Young from the first. On the strength of this, says he, "I ventured to predict, that if the reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums surrounding it, the central spot would be white; and I have now the pleasure of stating, that I have fully verified this prediction by interposing a drop of oil of sassafras between a prism of flint-glass and a lens of crown-glass."

The inaccuracy of his calculations consisted in his considering the external fringe of shadows to be produced by the interference of a ray *reflected* from the edge of the object, with a ray which passes clear of it; instead of supposing all the parts of the wave of light to corroborate or interfere with one another. The mathematical treatment of the question on the latter hypothesis was by no means easy. Young was a mathematician of considerable power in the solution of the problems which came before him: though his methods possessed none of the analytical elegance which, in his time, had become general in France. But it does not appear that he ever solved the problem of undulations as applied to fringes,

with its true conditions. He did, however, rectify his conceptions of the nature of the interference; and we may add, that the numerical error of the consequences of the defective hypothesis is not such as to prevent their confirming the undulatory theory.

But though this theory was thus so powerfully recommended by experiment and calculation, it met with little favour in the scientific world. Perhaps this will be in some measure accounted for, when we come, in the next chapter, to speak of the mode of its reception by the supposed judges of science and letters. Its author went on labouring at the completion and application of the theory in other parts of the subject; but his extraordinary success in unravelling the complex phenomena of which we have been speaking, appears to have excited none of the notice and admiration which properly belonged to it, till Fresnel's Memoir "on Diffraction" was delivered to the Institute, in October, 1815.

MM. Arago and Poinsot were commissioned to make a report upon this Memoir; and the former of these philosophers threw himself upon the subject with a zeal and intelligence which peculiarly belonged to him. He verified the laws announced by Fresnel: "laws," he says, "which appear to me destined to make an epoch in science." He then cast a rapid glance at the history of the subject, and recognised, at once, the place which Young occupied in it. Grimaldi, Newton, Maraldi, he states, had observed

the facts, and tried in vain to reduce them to rule or cause. "Such[2] was the state of our knowledge on this difficult question, when Dr. Thomas Young made the very remarkable experiment which is described in the Philosophical Transactions for 1803;" namely, that to obliterate all the bands within the shadow, we need only stop the ray which is going to graze, or has grazed, one border of the object. To this, Arago added the important observation, that the same obliteration takes place, if we stop the rays with a transparent plate; except the plate be very thin, in which case the bands are displaced, and not extinguished. "Fresnel," says he, "guessed the effect which a thin plate would produce, when I had told him of the effect of a thick glass." Fresnel himself declares[3] that he was not, at the time, aware of Young's previous labours. After stating nearly the same reasonings concerning fringes which Young had put forward in 1801, he adds, "it is therefore the meeting, the actual crossing of the rays, which produces the fringes. This consequence, which is only, so to speak, the translation of the phenomenon, seems to me entirely opposed to the hypothesis of emission, and confirms the system which makes light consist in the vibrations of a peculiar fluid." And thus the Principle of Interferences, and the theory of undulations, so far as that principle depends upon the theory, was a second time established by Fresnel in France, fourteen years after it had been

[2] Ann. Chim. 1815, Febr. [3] Ib. tom. xvii. p. 402.

discovered, fully proved, and repeatedly published by Young in England.

In this Memoir of Fresnel's, he takes very nearly the same course as Young had done; considering the interference of the direct light with that reflected at the edge, as the cause of the external fringes; and he observes, that in this reflection it is necessary to suppose half an undulation lost: but a few years later, he considered the propagation of undulations in a more true and general manner, and obtained the solution of this difficulty of the half-undulation. His more complete Memoir on Diffraction was delivered to the Institute of France, July 29, 1818; and had the prize awarded it in 1819[4]: but by the delays which at that period occurred in the publication of the Parisian Academical Transactions, it was not published[5] till 1826, when the theory was no longer generally doubtful or unknown in the scientific world. In this Memoir, Fresnel observes, that we must consider the effect of *every portion* of a wave of light upon a distant point, and must, on this principle, find the illumination produced by any number of such waves together. Hence, in general, the process of integration is requisite; and though the integrals which here offer themselves are of a new and difficult kind, he succeeded in making the calculation for the cases in which he experimented. His Table of the Correspondences of Theory and Observation[6], is very remarkable for the closeness of the

[4] Ann. Chim. May, 1819.

[5] Mém. Inst. for 1821-2. [6] Ib. p. 420—424.

agreement; the errors being generally less than one hundredth of the whole, in the distances of the black bands. He justly adds, "A more striking agreement could not be expected between experiment and theory. If we compare the smallness of the differences with the extent of the breadths measured; and if we remark the great variations which a and b (the distance of the object from the luminous point and from the screen,) have received in the different observations, we shall find it difficult not to regard the integral which has led us to these results as the faithful expression of the law of the phenomena."

A mathematical theory, applied, with this success, to a variety of cases of very different kinds, could not now fail to take strong hold of the attention of mathematicians; and accordingly, from this time, the undulatory doctrine of diffraction has been generally assented to, and the mathematical difficulties which it involves, have been duly studied and struggled with.

Among the remarkable earlier applications of the undulatory doctrine to diffraction, we may notice those of Joseph Fraunhofer, a mathematical optician of Munich. He made a great number of experiments on the shadows produced by small holes, and groups of small holes, very near each other. These were published[7] in his *New Modifications of Light*, in 1823. The greater part of this Memoir is employed in tracing the laws of phenomena of the

[7] In Schumacher's Astronomiche Abhandlungen, in French; earlier, in German.

extremely complex and splendid appearances which he obtained; but at the conclusion he observes, "It is remarkable that the laws of the reciprocal influence and of the diffraction of the rays, can be deduced from the principles of the undulatory theory: knowing the conditions, we may, by means of an extremely simple equation, determine the extent of a luminous wave for each of the different colours; and in every case, the calculation corresponds with observation." This mention of "an extremely simple equation," appears to imply that he employed only Young's and Fresnel's earlier mode of calculating interferences, by considering two portions of light, and not the method of integration. Both from the late period at which they were published, and from the absence of mathematical details, Fraunhofer's labours had not any strong influence on the establishment of the undulatory theory; although they are excellent verifications of it, both from the goodness of the observations, and the complexity and beauty of the phenomena.

We have now to consider the progress of the undulatory theory in another of its departments, according to the division already stated.

Sect. 3.—*Explanation of Double Refraction by the Undulatory Theory.*

We have traced the history of the undulatory theory applied to diffraction, into the period when Young ne to have Fresnel for his fellow-labourer. But

in the mean time, Young had considered the theory in its reference to other phenomena, and especially to those of *double refraction.*

In this case, indeed, Huyghens's explanation of the facts of Iceland spar, by means of spheroidal undulations, was so complete, and had been so fully confirmed by the measurements of Haüy and Wollaston, that little remained to be done, except to connect the Huyghenian hypothesis with the mechanical views belonging to the theory, and to extend his law to other cases. The former part of this task Young executed, by remarking that we may conceive the *elasticity* of the crystal, on which the velocity of propagation of the luminiferous undulation depends, to be different, in the direction of the crystallographic axis, and in the direction of the planes at right angles to this axis; and from such a difference, he deduces the existence of spheroidal undulations. This suggestion appeared in the Quarterly Review for November, 1809, in a critique upon an attempt of Laplace to account for the same phenomenon. Laplace had proposed to reduce the double refraction of such crystals as Iceland spar, to his favourite machinery of forces which are sensible at small distances only. The peculiar forces which produce the effect in this case, he conceives to emanate from the crystallographic axis; so that the velocity of light within the crystal will depend only on the situation of the ray with respect to this axis. But the establishment of this condition is, as Young ob-

serves, the main difficulty of the problem. How are we to conceive refracting forces, independent of the surface of the refracting medium, and regulated only by a certain internal line? Moreover, the law of force which Laplace was obliged to assume, namely, that it varied as the square of the sine of the angle which the ray made with the axis, could hardly be reconciled with mechanical principles. In the critique just mentioned, Young appears to feel that the undulatory theory, and perhaps he himself, had not received justice at the hands of men of science; he complains of a person so eminent in the world of science as Laplace then was, employing his influence in propagating error, and disregarding the extraordinary confirmations which the Huyghenian theory had recently received.

The extension of this view, of the different elasticity of crystals in different directions, to other than uniaxal crystals, was a more complex and difficult problem. The general notion was perhaps obvious, after what Young had done; but its application and verification involved mathematical calculations of great generality, and required also very exact experiments. In fact, this application was not made till Fresnel, a pupil of the Polytechnic School, brought the resources of the modern analysis to bear upon the problem;—till the phenomena of dipolarized light presented the properties of biaxal crystals in a vast variety of forms;—and till the theory received its grand impulse by the combination of the explana-

tion of polarization with that of double refraction. To the history of this last-mentioned great step we now proceed.

Sect. 4.—*Explanation of Polarization by the Undulatory Theory.*

EVEN while the only phenomena of *polarization* which were known were those which affect the two images in Iceland spar, the difficulty which these facts seemed at first to throw in the way of the undulatory theory was felt and acknowledged by Young. Malus's discovery of polarization by reflection increased the difficulty, and this Young did not attempt to conceal. In his review of the papers containing this discovery[8] he says, "The discovery related in these papers appears to us to be by far the most important and interesting which has been made in France concerning the properties of light, at least since the time of Huyghens; and it is so much the more deserving of notice, as it greatly influences the general balance of evidence in the comparison of the undulatory and projectile theories of the nature of light." He then proceeds to point out the main features in this comparison, claiming justly a great advantage for the theory of undulations on the two points we have been considering, the phenomena of diffraction and of double refraction. And he adds, with reference to the embarrassment introduced by

[8] Quart. Rev. May, 1810.

polarization, that we are not to expect the course of scientific discovery to run smooth and uninterrupted; but that we are to lay our account with partial obscurity and seeming contradiction, which we may hope that time and enlarged research will dissipate. And thus he steadfastly held, with no blind prejudice, but with unshaken confidence, his great philosophical trust, the fortunes of the undulatory theory. It is here, after the difficulties of polarization had come into view, and before their solution had been discovered, that we may place the darkest time of the history of the theory; and at this period Young was alone in the field.

It does not appear that the light dawned upon him for some years. In the mean time, Young found that his theory would explain dipolarized colours; and he had the satisfaction to see Fresnel re-discover, and Arago adopt, his views on diffraction. He became engaged in friendly intercourse with the latter philosopher, who visited him in England in 1816. On January the 12th, 1817, in writing to this gentleman, among other remarks on the subject of optics, he says, "I have also been reflecting on the possibility of giving an imperfect explanation of the affection of light which constistitutes polarization, without departing from the genuine doctrine of undulation." He then proceeds to suggest the possibility of "a *transverse* vibration, propagated in the direction of the radius, the motions of the particles being in a certain constant direction with respect to that radius; and this," he adds, "is

polarization." From his further explanation of his views, it appears that he conceived the motions of the particles to be oblique to the direction of the ray, and not perpendicular, as the theory was afterwards framed; but still, here was the essential condition for the explanation of the facts of polarization,—the transverse nature of the vibrations. This idea at once made it possible to conceive how the rays of light could have sides; for the direction in which the vibration was transverse to the ray, might be marked by peculiar properties. And after the idea was once started, it was comparatively easy for men like Young and Fresnel to pursue and modify it till it assumed its true and distinct form.

We may judge of the difficulty of taking firmly hold of the conception of transverse vibrations of the ether, as those which constitute light, by observing how long the great philosophers of whom we are speaking lingered within reach of it, before they ventured to grasp it. Fresnel says, in 1821, "When M. Arago and I had remarked (in 1816) that two rays polarized at right angles always give the same quantity of light by their union, I thought this might be explained by supposing the vibrations to be transverse, and to be at right angles when the rays are polarized at right angles. But this supposition was so contrary to the received ideas on the nature of the vibrations of elastic fluids," that Fresnel hesitated to adopt it till he could reconcile it better to his mechanical notions. "Mr. Young, more bold in his conjectures, and less confiding in the views of

geometers, published it before me, though perhaps he thought it after me." And Arago was afterwards wont to relate[9] that when he and Fresnel had obtained their joint experimental results, of the non-interference of oppositely polarized pencils, and when Fresnel pointed out that transverse vibrations were the only possible translation of this fact into the undulatory theory, he himself protested that he had not courage to publish such a conception; and accordingly, the second part of the Memoir was published in Fresnel's name alone. What renders this more remarkable is, that it occurred when Arago had in his possession the very letter of Young, in which he proposed the same suggestion.

Young's first published statement of the doctrine of transverse vibrations was given in the explanation of the phenomena of dipolarization, of which we shall have to speak in the next Section. But the primary and immense value of this conception, as a step in the progress of the undulatory theory, was the connexion which it established between polarization and double refraction; for it held forth a promise of accounting for polarization, if any conditions could be found, which might determine the direction of the transverse vibrations. The analysis of these conditions is, in a great measure, the work of Fresnel; a task of profound philosophical sagacity and mathematical skill.

Since the double refraction of uniaxal crystals

[9] I take the liberty of stating this from personal knowledge.

could be explained by undulations of the form of a spheroid, it was perhaps not difficult to conjecture that the undulations of biaxal crystals would be accounted for by undulations of the form of an ellipsoid, which differs from the spheroid in having its three axes unequal, instead of two only; and consequently has that very relation to the other, in respect of symmetry, which the crystalline and optical phenomena have. Or, again, instead of supposing two different degrees of elasticity in different directions, we may suppose three such different degrees in directions at right angles to each other. This kind of generalisation was tolerably obvious to a practised mathematician.

But what shall call into play all these elasticities at once, and produce waves governed by each of them? And what shall explain the different polarization of the rays which these separate waves carry with them? These were difficult questions, to the solution of which mathematical calculation had hitherto been unable to offer any aid.

It was here that the conception of transverse vibrations came in, like a beam of sunlight, to disclose the possibility of a mechanical connexion of all these facts. If transverse vibrations, travelling through a uniform medium, come to a medium not uniform, but constituted so that the elasticity shall be different in different directions, in the manner we have described, what will be the course and condition of the waves in the second medium? Will the effects of such waves agree with the phenomena

of doubly-refracted light in biaxal crystals? Here was a problem, striking to the mathematician for its generality and difficulty, and of deep interest to the physical philosopher, because the fate of a great theory depended upon its solution.

The solution, obtained by great mathematical skill, was laid before the French Institute by Fresnel in November, 1821, and was carried further in two Memoirs presented in 1822. Its import is very curious. The undulations which, coming from a distant centre, fall upon such a medium as we have described, are, it appears from the principles of mechanics, propagated in a manner quite different from anything which had been anticipated. The "surface of the waves" (that is, the surface which would bound undulations diverging from a point,) is a very complex, yet symmetrical curve surface; which, in the case of uniaxal crystals, resolves itself into a sphere and a spheroid; but which, in general, forms a continuous double envelope of the central point to which it belongs, intersecting itself, and returning into itself. The directions of the rays are determined by this curve surface in biaxal crystals, as in uniaxal crystals they are determined by the sphere and the spheroid; and the result is, that in biaxal crystals, *both* rays suffer *extraordinary* refraction according to determinate laws. And the positions of the planes of polarization of the two rays follow from the same investigation; the plane of polarization in this case being supposed to be that which is perpendicular to the transverse vibrations. Now it

appeared that the polarization of the two rays, as determined by Fresnel's theory, would be in directions, not indeed exactly accordant with the law deduced by Biot from experiment, but deviating so little from those directions, that there could be small doubt that the empirical formula was wrong, and the theoretical one right.

The theory was further confirmed by an experiment showing that, in a biaxal crystal (topaz), neither of the rays was refracted according to the ordinary law, though it had hitherto been supposed that one of them was so;—a natural inaccuracy, since the error was small[10]. Thus this beautiful theory corrected, while it explained, the best of the observations which had previously been made; and offered itself to mathematicians with an almost irresistible power of conviction. The explanation of laws so strange as those of double refraction and polarization, by the same general and symmetrical theory, could not result from anything but the truth of the theory.

"Long," says Fresnel[11], "before I had conceived this theory, I had convinced myself, by a pure contemplation of the facts, that it was not possible to discover the true explanation of double refraction, without explaining, at the same time, the phenomena of polarization, which always goes along with it; and accordingly, it was after having found what mode of vibration constituted polarization, that I caught

[10] An. Ch. xxviii. p. 264.

[11] Sur la Double Ref., Mém. Ins. 1826, p. 174.

sight of the mechanical causes of double refraction."

Having thus got possession of the principle of the mechanism of polarization, Fresnel proceeded to apply it to the other cases of polarized light, with a rapidity and sagacity which reminds us of the spirit in which Newton traced out the consequences of the principle of universal gravitation. In the execution of his task, indeed, Fresnel was forced upon several precarious assumptions, which make, even yet, a wide difference between the theory of gravitation and that of light. But the mode in which these were confirmed by experiment, compels us to admire the apparently happy boldness of the calculator.

The subject of *polarization by reflection* was one of those which seemed most untractable; but, by means of various artifices and conjectures, it was broken up and subdued. Fresnel began with the simplest case, the reflection of light polarized in the plane of reflection; which he solved by means of the laws of collision of elastic bodies. He then took the reflection of light polarized perpendicularly to this plane; and here, adding to the general mechanical principles an empirical assumption, that the communication of the resolved motion parallel to the refracting surface, takes place according to the laws of elastic bodies, he obtains his formula. These results were capable of comparison with experiment; and the comparison, when made by Arago, confirmed the formulæ. They accounted, too, for Brewster's law concerning the angle of polarization; and this

could not but be looked upon as a striking evidence of their having some real foundation. Another artifice which Fresnel and Arago employed, in order to trace the effect of reflection upon common light, was to use a ray polarized in a plane making half a right angle with the plane of reflection; for the quantities of the oppositely[12] polarized light in such an incident ray are equal, as they are in common light; but the relative quantities of the oppositely polarized light in the reflected ray are indicated by the new plane of polarization; and thus these relative quantities become known for the case of common light. The results thus obtained were also confirmed by facts; and in this manner, all that was doubtful in the process of Fresnel's reasoning, seemed to be authorized by its application to real cases.

These investigations were published[13] in 1821. In succeeding years, Fresnel undertook to extend the application of his formulæ to a case in which they ceased to have a meaning, or, in the language of mathematicians, became imaginary; namely, to the case of internal reflection. It may seem strange to those who are not mathematicians, but it is undoubtedly true, that in many cases in which the solution of a problem directs impossible operations to be performed, these directions may be so interpreted as to point out a true solution of the question. Such an

[12] It will be recollected all along, that *oppositely* polarized rays are those which are polarized in two planes *perpendicular* to each other.

[13] An. Chim. t. xvii.

interpretation Fresnel attempted[14] in the case of which we now speak; and the result at which he arrived was, that the reflection of light through a rhomb of glass of a certain form (since called *Fresnel's rhomb*), would produce a polarization of a kind altogether different from those which his theory had previously considered, namely, that kind which we have spoken of as *circular polarization*. The complete confirmation of this curious and unexpected result by trial, is another of the extraordinary triumphs which have distinguished the history of the theory at every step since the commencement of Fresnel's labours.

But anything further which has been done in this way, may be treated of more properly in relating the verification of the theory. And we have still to speak of the most numerous and varied class of facts to which rival theories of light were applied, and of the establishment of the undulatory doctrine in reference to that department; I mean the phenomena of depolarized, or rather, as I have already said, *di*polarized light.

Sect. 5.—*Explanation of Dipolarization by the Undulatory Theory.*

When Arago, in 1811, had discovered the colours produced by polarized light passing through certain crystals[15], it was natural that attempts should be

[14] Bullet. des. Sc. Feb. 1823. [15] See p. 384.

made to reduce them to theory. Biot, animated by the success of Malus in detecting the laws of double refraction, and Young, knowing the resources of his own theory, were the first persons to enter upon this undertaking. Biot's theory, though in the end displaced by its rival, is well worth notice in the history of the subject. It was what he called the doctrine of *moveable polarization*. He conceived that when the molecules of light pass through thin crystalline plates, the plane of polarization undergoes an oscillation which carries it backwards and forwards through a certain angle, namely, twice the angle contained between the original plane of polarization and the principal section of the crystal. The intervals which this oscillation occupies are lengths of the path of the ray, very minute, and different for different colours, like Newton's fits of easy transmission; on which model, indeed, the new theory was evidently framed[16]. The colours produced in the phenomena of dipolarization really do depend, in a periodical manner, on the length of the path of the light through the crystal, and a theory such as Biot's was capable of being modified, and was modified, so as to include the leading features of the facts as then known; but many of its conditions being founded on special circumstances in the experiments, and not on the real conditions of nature, there were in it several incon-

[16] See Arago and Biot's Memoirs, Mém. Inst. for 1811; the whole volume for 1812 is a Memoir of Biot's (published 1814); also Mém. Inst. for 1817; Biot's Mem. read in 1818, published in 1819, and for 1818.

gruities, as well as the general defect of its being an arbitrary and unconnected hypothesis.

Young's mode of accounting for the brilliant phenomena of dipolarization appeared in the "Quarterly Review" for 1814. After noticing the discoveries of Arago, Brewster, and Biot, he adds, "We have no doubt that the surprise of these gentlemen will be as great as our own satisfaction in finding that they are perfectly reducible, like other causes of recurrent colours, to the general laws of the interference of light which have been established in this country;" giving a reference to his former statements. The results are explained by the interference of the ordinary and extraordinary ray. But, as Arago properly observes, in his account of this matter[17], "It must, however, be added that Dr. Young had not explained either in what circumstances the interference of the rays can take place, nor why we see no colours unless the crystallized plates are exposed to light previously polarized." The explanation of these circumstances depends on the laws of interference of polarized light which Arago and Fresnel had established in 1816. They had proved, by direct experiment, that when polarized light was treated so as to bring into view the most marked phenomena of interference, namely, the bands of shadows; pencils of light which have a common origin, and which are polarized in the parallel planes, interfere completely, while those

[17] Enc. Brit. Supp. art. Polarization.

which are polarized in *opposite* (that is, perpendicular,) planes do not interfere at all[18]. Taking these principles into the account, Fresnel explained very completely all the circumstances of colours produced by crystallized plates; showing the necessity of the *polarization* in the first instance; the *dipolarizing* effect of the crystal; and the office of the *analysing plate*, by which certain portions of each of the two rays in the crystal are made to interfere and produce colour. This he did, as he says[19], without being aware, till Arago told him, that Young had, to some extent, anticipated him.

When we look at the history of the emission-theory of light, we see exactly what we may consider as the natural course of things in the career of a false theory. Such a theory may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition,—an addition to the machinery; and as observation goes on, these incoherent appendages accumulate, till they overwhelm and upset the original frame-work. Such was the history of the hypothesis of solid epicycles; such has been the history of the hypothesis of the material emission of light. In its simple form, it explained reflection and refraction; but the colours of thin plates added to it the fits of easy transmission and reflection; the phenomena of diffraction further invested the particles with complex laws of attraction and repulsion;

[18] Ann. Chim. tom. x. [19] Ib. tom. xvii. p. 402.

polarization gave them sides; double refraction subjected them to peculiar forces emanating from the axes of crystals; finally, dipolarization loaded them with the complex and unconnected contrivance of moveable polarization; and even when all this had been assumed, additional mechanism was wanting. There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters: the philosopher builds the machine, but its parts do not fit; they hold together only while he presses them: this is not the character of truth.

In the undulatory theory, on the other hand, all tends to unity and simplicity. We explain reflection and refraction by undulations; when we come to thin plates, the requisite "fits," are already involved in our fundamental hypothesis, for they are the length of an undulation: the phenomena of diffraction also require such intervals; and the intervals thus required agree exactly with the others in magnitude, so that no new property is needed. Polarization for a moment checks us; but not long; for the direction of our vibrations is hitherto arbitrary;—we allow polarization to decide it. Having done this for the sake of polarization, we find that it also answers an entirely different purpose, that of giving the law of double refraction. Truth may give rise to such a coincidence; falsehood cannot. But the phenomena became more numerous, more various, more strange:—no matter: the theory is equal to them all. It makes not a

single new physical hypothesis; but out of its original stock of principles it educes the counterpart of all that observation shows. It accounts for, explains, simplifies, the most entangled cases; corrects known laws and facts; predicts and discloses unknown ones; becomes the guide of its former teacher, observation; and, enlightened by mechanical conceptions, acquires an insight which pierces through shape and colour to force and cause.

We thus reach the philosophical *moral* of this history, so important in reference to our purpose; and here we shall close the account of the discovery and promulgation of the undulatory theory. Any further steps in its developement and extension, may with propriety be noticed in the ensuing chapters.

CHAPTER XII.

SEQUEL TO THE EPOCH OF YOUNG AND FRESNEL. RECEPTION OF THE UNDULATORY THEORY.

WHEN Young, in 1800, published his assertion of the Principle of Interferences, as the true theory of optical phenomena, the condition of England was not very favourable to a fair appreciation of the value of the new opinion. The men of science were strongly pre-occupied in favour of the doctrine of emission, not only from a national interest in Newton's glory, and a natural reverence for his authority, but also from deference towards the geometers of France, who were looked up to as our masters in the application of mathematics to physics, and who were understood to be Newtonians in this as in other subjects. A general tendency to an atomic philosophy, which had begun to appear from the time of Newton, operated powerfully; and the hypothesis of emission was so easily conceived, that, when recommended by high authority, it easily became popular; while the hypothesis of luminiferous undulations, unavoidably difficult to comprehend, even by the aid of steady thought, was neglected, and all but forgotten.

Yet the reception which Young's opinions met with was more harsh than we might have expected,

even taking into account all these considerations. There was in England no visible body of men, fitted by their knowledge and character to pronounce judgment on such a question, or to give the proper impulse and bias to public opinion. The Royal Society, for instance, had not, for a long time, by custom or institution, possessed or aimed at such functions. The writers of "Reviews" alone, self-constituted and secret tribunals, claimed this kind of authority. Among these publications, by far the most distinguished about this period was the Edinburgh Review; and, including among its contributors men of eminent science and great talents, employing also a robust and poignant style of writing (often certainly in a very unfair manner), it naturally exercised great influence. On abstruse subjects, accessible to few, more than on others, the opinions and feelings expressed in a Review must be those of the individual reviewer. The criticism on some of Young's early papers on optics was written by Mr. (afterwards Lord) Brougham, who, as we have seen, had experimented on diffraction, following the Newtonian view, that of inflexion. Mr. Brougham was perhaps at this time young enough[1] to be somewhat intoxicated with the appearance of judicial authority in matters of science, which his office of anonymous reviewer gave him: and even in middle-life, he was sometimes considered to be prone to indulge himself in severe and sarcastic expres-

[1] His age was twenty-four.

sions. In January, 1803, was published[1] his critique on Dr. Young's Bakerian Lecture, "On the Theory of Light and Colours," in which lecture the doctrine of undulations and the law of interferences was maintained. This critique was an uninterrupted strain of blame and rebuke. "This paper," the reviewer said, "contains nothing which deserves the name either of experiment or discovery." He charged the writer with "dangerous relaxations of the principles of physical logic." "We wish," he cried, "to recall philosophers to the strict and severe methods of investigation," describing them as those pointed out by Bacon, Newton, and the like. Finally, Dr. Young's speculations were spoken of as a hypothesis, which is a mere work of fancy; and the critic added, "we cannot conclude our review without entreating the attention of the Royal Society, which has admitted of late so many hasty and unsubstantial papers into its Transactions;" which habit he urged them to reform. The same aversion to the undulatory theory appears soon after in another article by the same reviewer, on the subject of Wollaston's measures of the refraction of Iceland spar; he says, "We are much disappointed to find that so acute and ingenious an experimentalist should have adopted the wild optical theory of vibrations." The reviewer showed ignorance as well as prejudice in the course of his remarks; and Young drew up an answer, which was ably written, but being pub-

[1] Edin. Rev. vol. i. p. 450.

lished separately had little circulation. We can hardly doubt that these Edinburgh reviews had their effect in confirming the general disposition to reject the undulatory theory.

We may add, however, that Young's mode of presenting his opinions was not the most likely to win them favour; for his mathematical reasonings placed them out of the reach of popular readers, while the want of symmetry and system in his symbolical calculations, deprived them of attractiveness for the mathematician. He himself gave a very just criticism of his own style of writing, in speaking on another of his works[3]: "The mathematical reasoning, for want of mathematical symbols, was not understood, even by tolerable mathematicians. From a dislike of the affectation of algebraical formality which he had observed in some foreign authors, he was led into something like an affectation of simplicity, which was equally inconvenient to a scientific reader."

Young appears to have been aware of his own deficiency in the power of drawing public favour, or even notice, to his discoveries. In 1802, Davy writes to a friend, "Have you seen the theory of my colleague, Dr. Young, on the undulations of an ethereal medium as the cause of light? It is not likely to be a popular hypothesis, after what has been said by Newton concerning it. He would be very much flattered if you could offer any observations

[3] See Life of Young, p. 54.

upon it, *whether for or against it.*" Young naturally felt confident in his power of refuting objections, and wanted only the opportunity of a public combat.

Dr. Brewster, who was, at this period, enriching optical knowledge with so vast a train of new phenomena and laws, shared the general aversion to the undulatory theory, which, indeed, he hardly overcame thirty years later. Dr. Wollaston was a person whose character led him to look long at the laws of phenomena, before he attempted to determine their causes; and it does not appear that he had decided the claims of the rival theories in his own mind. Herschel (I now speak of the son,) had at first the general mathematical prejudice in favour of the emission doctrine. Even when he had himself studied and extended the laws of dipolarized phenomena, he translated them into the language of the theory of moveable polarization. In 1819, he refers to, and corrects, this theory; and says, it is now "relieved from every difficulty, and entitled to rank with the fits of easy transmission and reflection as a general and simple physical law:" a just judgment, but one which now conveys less of praise than he then intended. At a later period, he remarked that we cannot be certain that if the theory of emission had been as much cultivated as that of undulation, it might not have been as successful; an opinion which was certainly untenable after the fair trial of the two theories in the case of diffraction, and extravagant after Fresnel's beautiful explanation of double refraction and polarization. Even in 1827, in a Treatise

on Light, published in the Encyclopædia Metropolitana, he gives a section to the calculations of the Newtonian theory; and appears to consider the rivalry of the theories as still subsisting. But yet he there speaks with a proper appreciation of the advantages of the new doctrine. After tracing the prelude to it, he says, "But the unpursued speculations of Newton, and the opinions of Hooke, however distinct, must not be put in competition, and, indeed, ought scarcely to be mentioned, with the elegant, simple, and comprehensive theory of Young,—a theory which, if not founded in nature, is certainly one of the happiest fictions that the genius of man ever invented to grasp together natural phenomena, which, at their first discovery, seemed in irreconcileable opposition to it. It is, in fact, in all its applications and details, one succession of *felicities;* insomuch, that we may almost be induced to say, if it be not true, it deserves to be so."

In France, Young's theory was little noticed or known, except perhaps by Arago, till it was revived by Fresnel. And though Fresnel's assertion of the undulatory theory was not so rudely received as Young's had been, it met with no small opposition from the older mathematicians, and made its way slowly to the notice and comprehension of men of science. Arago would perhaps have at once adopted the conception of transverse vibrations, when it was suggested by his fellow-labourer, Fresnel, if it had not been that he was a member of the Insti-

tute, and had to bear the brunt of the war, in the frequent discussions on the undulatory theory; to which theory Laplace, and other leading members, were so vehemently opposed, that they would not even listen with toleration to the arguments in its favour. I do not know how far influences of this kind might operate in producing the delays which took place in the publication of Fresnel's papers. We have seen that he arrived at the conception of transverse vibrations in 1816, as the true key to the understanding of polarization. In 1817 and 1818, in a memoir read to the Institute, he analyzed and explained the perplexing phenomena of quartz, which he ascribed to a *circular polarization*. This memoir had not been printed, nor any extract from it inserted in the Scientific Journals, in 1822, when he confirmed his views by further experiments[4]. His remarkable memoir, which solved the extraordinary and capital problem of the connexion of double refraction and crystallization, though written in 1821, was not published till 1827. He appears by this time to have sought other channels of publication. In 1822, he gave[5], in the *Annales de Chimie et de Physique*, an explanation of refraction on the principles of the undulatory theory; alleging, as the reason for doing so, that the theory was still little known. And in succeeding years there appeared in the same work, his theory of reflection. His memoir

[4] Hersch. Light, p. 539.

[5] Ann. de Chim. 1822, tom. xxi. p. 235.

on this subject (*Mémoire sur la Loi des Modifications que la Réflexion imprime à la Lumière Polarisée,*) was read to the Academy of Sciences in 1823. But the original paper was mislaid, and, for a time, supposed to be lost; it has since been recovered among the papers of M. Fourier, and printed in the eleventh volume of the Memoirs of the Academy[6]. Some of the speculations to which he refers, as communicated to the Academy, have never yet appeared[7].

Still Fresnel's labours were, from the first, duly appreciated by some of the most eminent of his countrymen. His Memoir on Diffraction was, as we have seen, crowned in 1819: and, in 1822, a Report upon his Memoir on Double Refraction was drawn up by a commission, consisting of Fourier, Ampère, and Arago. In this Report[8] Fresnel's theory is spoken of as confirmed by the most delicate tests. The reporters add, respecting his "theoretical ideas on the particular kind of undulations which, according to him, constitute light," that "it would be impossible for them to pronounce at present a decided judgment," but that "they have not thought it right to delay any longer making known a work of which the difficulty is attested by the fruitless efforts of the most skilful philosophers, and in which are exhibited, in the same brilliant degree, the talent for experiment and the spirit of invention."

[6] Lloyd, Report on Optics, p. 363. (Fourth Rep. of Brit. Asso.)
[7] Ib. p. 316, *note.* [8] Ann. Chim. tom. xx. p. 343.

In the mean time, however, a controversy between the theory of undulations and the theory of moveable polarization which Biot had proposed with a view of accounting for the colours produced by dipolarizing crystals, had occurred among the French men of science; and this dispute was carried on, we may now venture to say, with very unnecessary acrimony. It is clear that in some main features the two theories coincide; the intervals of interference in the one theory being represented by the intervals of the oscillations in the other. But these intervals in Biot's explanation were arbitrary hypotheses, suggested by these very facts themselves; in Fresnel's theory, they were essential parts of the general scheme. Biot, indeed, does not appear to have been averse from a coalition; for he allowed[9] to Fresnel that "the theory of undulations took the phenomena at a higher point and carried them further." And Biot could hardly have dissented from Arago's account of the matter, that Fresnel's views "*linked together*[10]" the oscillations of moveable polarization. But Fresnel, whose hypothesis was all of one piece, could give up no part of it, although he allowed the usefulness of Biot's formulæ. Yet Biot's speculations fell in better with the views of the leading mathematicians of Paris. We may consider as evidence of the favour with which they were looked upon, the large space they occupy in the volumes of the Academy for 1811, 1812, 1817, and

[9] Ann. Chim. tom. xvii. p. 251. [10] "Nouait."

1818. In 1812, the entire volume is filled with a memoir of Biot's on the subject of moveable polarization. This doctrine also had some advantage in coming early before the world in a didactic form, in his *Traité de Physique*, which was published in 1816, and was the most complete treatise on general physics which had appeared up to that time. In this and others of this author's writings, he expresses facts so entirely in the terms of his own hypothesis, that it is difficult to separate the two. In the sequel Arago was the most prominent of Biot's opponents; and in his report upon Fresnel's memoir on the colours of crystalline plates, he exposed the weaknesses of the theory of moveable polarization with so much severity, that these two eminent philosophers became entirely estranged from each other. The details of this controversy need not occupy us; but we may observe that this may be considered as the last struggle in favour of the theory of emission among mathematicians of eminence. After this crisis of the war, the theory of moveable polarization lost its ground; and the explanations of the undulatory theory, and the calculations belonging to it, being published in the *Annales de Chimie et de Physique*, of which Arago was one of the conductors, soon diffused it over Europe.

It was probably in consequence of the delays to which we have referred, in the publication of Fresnel's memoirs, that as late as December, 1826, the Imperial Academy at St. Petersburg proposed, as one of their prize-questions for the two following

years, this,—"To deliver the optical system of waves from all the objections which have (as it appears) with justice been urged against it, and to apply it to the polarization and double refraction of light." In the programme to this announcement, Fresnel's researches on the subject are not alluded to, though his memoir on diffraction is noticed; they were, therefore, probably not known to the Russian Academy.

Young was always looked upon as a person of marvellous variety of attainments and extent of knowledge; but during his life he hardly held that elevated place among great discoverers which posterity will probably assign him. In 1802, he was constituted foreign secretary of the Royal Society, an office which he held during life; in 1827 he was elected one of the eight foreign members of the Institute of France; perhaps the greatest honour which men of science usually receive. The fortune of his life in some other respects was of a mingled complexion. His profession of a physician occupied, sufficiently to fetter, without rewarding him; while he was lecturer at the Royal Institution, he was, in his lectures, too profound to be popular; and his office of Superintendent of the Nautical Almanac subjected him to much minute labour, and many petulant attacks of pamphleteers. On the other hand, he had a leading part in the discovery of the long-sought key to the Egyptian hieroglyphics; and thus the age which was marked by two great discoveries, one in science and one in literature, owed them both in a great measure to him. Dr. Young

died in 1829, when he had scarcely completed his fifty-sixth year. Fresnel was snatched from science still more prematurely, dying, in 1827, at the early age of thirty-nine.

We need not say that both these great philosophers possessed, in an eminent degree, the leading characteristics of the discoverer's mind, perfect clearness of view, rich fertility of invention, and intense love of knowledge. We cannot read without great interest a letter of Fresnel to Young[11], in November, 1824: "For a long time that sensibility, or that vanity, which people call love of glory, is much blunted in me. I labour much less to catch the suffrages of the public, than to obtain an inward approval which has always been the sweetest reward of my efforts. Without doubt I have often wanted the spur of vanity to excite me to pursue my researches in moments of disgust and discouragement. But all the compliments which I have received from MM. Arago, de la Place, or Biot, never gave me so much pleasure as the discovery of a theoretical truth, or the confirmation of a calculation by experiment."

Though Young and Fresnel were in years the contemporaries of many who are now alive, we must consider ourselves as standing towards them in the relation of posterity. The Epoch of induction in optics is past; we have now to trace the Verification and Application of the true theory.

[11] I am able to give this, and some other extracts, from the unedited correspondence of Young and Fresnel, by the kindness of Professor Peacock, of Trinity College, Cambridge, who is preparing for the press a Life of Dr. Young.

CHAPTER XIII.

CONFIRMATION AND EXTENSION OF THE UNDULATORY THEORY.

AFTER the undulatory theory had been developed in all its main features, by its great authors, Young and Fresnel, although it bore marks of truth which could hardly be fallacious, there was still here, as in the case of other great theories, a period in which difficulties were to be removed, objections answered, men's minds familiarised to the new conceptions thus presented to them; and in which, also, it might reasonably be expected that the theory would be extended to facts not at first included in its domain. This period is, indeed, that in which we are; and we might, perhaps with propriety, avoid the task of speaking of our living contemporaries. But it would be unjust to the theory not to notice some of the remarkable events, characteristic of such a period, which have already occurred; and this may be done very simply.

In the case of this great theory, as in that of gravitation, by far the most remarkable of these confirmatory researches were conducted by the authors of the discovery, especially Fresnel. And in looking at what he conceived and executed for this purpose, we are, it appears to me, strongly reminded of Newton, by the wonderful inventiveness and sagacity with which he devised experiments, and applied to them mathematical reasonings.

1. *Double Refraction of Compressed Glass.*—One of these confirmatory experiments was the production of double refraction by the *compression* of glass. Fresnel observes[1], that though Brewster had shown that glass under compression produced colours resembling those which are given by doubly-refracting crystals, "very skilful physicists had not considered those experiments as a sufficient proof of the bifurcation of the light." In the hypothesis of moveable polarization, it is added, there is no apparent connexion between these phenomena of coloration and double refraction; but on Young's theory, that the colours arise from two rays which have traversed the crystal with different velocities, it appears almost unavoidable to admit also a difference of path in the two rays.

"Though," he says, "I had long since adopted this opinion, it did not appear to me so completely demonstrated, that it was right to neglect an experimental verification of it;" and therefore, in 1819, he proceeded to satisfy himself of the fact, by the phenomena of diffraction. The trial left no doubt on the subject; but he still thought it would be interesting actually to produce two images in glass by compression; and by a highly-ingenious combination, calculated to exaggerate the effect of the double refraction, which is very feeble, even when the compression is most intense, he obtained two distinct images. This evidence of the dependence of dipolarizing structure upon a doubly-refracting

[1] Ann. de Chim. 1822, tom. xx. p. 377.

state of particles, thus excogitated out of the general theory, and verified by trial, may well be considered, as he says, "as a new occasion of proving the infallibility of the principle of interferences."

2. *Circular Polarization.*—Fresnel then turned his attention to another set of experiments, related to this indeed, but by a tie so recondite that nothing less than his clearness and acuteness of view could have detected any connexion. The optical properties of quartz had been perceived to be peculiar, from the period of the discovery of dipolarized colours by Arago and Biot. At the end of the notice just quoted, Fresnel says[2], "As soon as my occupations permit me, I propose to employ a pile of prisms similar to that which I have described, in order to study the double refraction of the rays which traverse crystals of quartz in the direction of the axis." He then ventures, without hesitation, to describe beforehand what the phenomena will be. In the *Bulletin des Sciences*[3] for December 1822, it is stated that experiment had confirmed what he had thus announced.

The phenomena are those which have since been spoken of as *circular polarization;* and the term first occurs in this notice[4]. They are very remarkable, both by their resemblances to, and their differences from, the phenomena of *plane-polarized* light. And the manner in which he was led to this anticipation of the facts is still more remarkable than the facts themselves. Having ascertained by observation that

[2] Ann. de Chim. 1822, tom. xx. p. 382.
[3] Ib. p. 191. [4] Ib. p. 194.

two differently-polarized rays, totally reflected at the internal surface of glass, suffer different *retardations* of their undulations, he applied the formulæ which he had obtained for the polarizing effect of reflection to this case. But in this case the formulæ expressed an impossibility; yet as algebraical formulæ, even in such cases, have often some meaning, "I interpreted," he says[5], "in the manner which appeared to me most natural and most probable, what the analysis indicated by this imaginary form;" and by such an interpretation he hence collected the law of the difference of undulation of the two rays. He was thus able to predict that by two internal reflections in a *rhomb*, or parallelopiped of glass, of a certain form and position, a polarized ray would acquire a circular undulation of its particles; and this constitution of the ray, it appeared, by reasoning further, would show itself by its possessing peculiar properties, partly the same as those of polarized light, and partly different. This extraordinary anticipation was exactly confirmed; and thus the apparently bold and strange guess of the author was fully justified, or at least assented to, even by the most cautious philosophers. "As I cannot appreciate the mathematical evidence for the nature of circular polarization," says Professor Airy[6], "I shall mention the experimental evidence on which I receive it." The conception has since been universally adopted.

But Fresnel, having thus obtained circularly-pola-

[5] Bullet. des Sc. 1823, p. 33.

[6] Camb. Trans. vol. iv. p. 81, 1831.

rized rays, saw that he could account for the phenomena of quartz, by supposing two circularly-polarized rays to pass, with different velocities, along the axis. The curious succession of colours, following each other in right-handed or left-handed circular order, of which we have already spoken, might thus be hypothetically explained.

But was this hypothesis of two circularly-polarized rays, travelling along the axis of such crystals, to be received, merely because it accounted for the phenomena? Fresnel's ingenuity again enabled him to avoid such a defect in theorising. If there were two such rays, they might be visibly separated[7] by the same artifice, of a pile of prisms properly achromatised, which he had used for compressed glass. The result was, that he did obtain a visible separation of the rays; and this result has since been confirmed by others, for instance, Professor Airy[8]. The rays were found to be in all respects identical with the circularly-polarized rays produced by the internal reflections in Fresnel's rhomb. This kind of double refraction gave a hypothetical explanation of the laws which Biot had obtained for the phenomena of this class; for example[9], the rule, that the deviation of the plane of polarization of the emergent ray is inversely as the square of the length of an undulation for each kind of rays. And thus the phenomena produced by light passing along the axis of quartz were reduced into complete conformity with the theory.

[7] Bull. des Sc. 1822, p. 193. [8] Camb. Tr. iv. p. 80.
[9] Bull. des Sc. 1822, p. 197.

3. *Elliptical Polarization in Quartz.*—We now come to one of the few additions to Fresnel's theory which have been shown to be necessary. He had accounted fully for the colours produced by the rays which travel *along the axis* of quartz crystals; and thus, for the colours and changes of the central spot which is produced when polarized light passes through a transverse plate of such crystals. But this central spot is surrounded by rings of colours. How is the theory to be extended to these?

This extension has been successfully made by Professor Airy[10]. His hypothesis is, that as rays passing along the axis of a quartz crystal are circularly polarized, rays which are oblique to the axis are elliptically polarized, the amount of ellipticity depending, in some unknown manner, upon the obliquity; and that each ray is separated by double refraction into two rays polarized elliptically; the one right-handed, the other left-handed. By means of these suppositions, he not only was enabled to account for the simple phenomena of single plates of quartz; but for many most complex and intricate appearances which arise from the superposition of two plates, and which at first sight might appear to defy all attempts to reduce them to law and symmetry; such as spirals, curves approaching to a square form, curves broken in four places. "I can hardly imagine," he says[11], very naturally, "that any other supposition would represent the phenomena to such extreme accuracy. I am not so much struck with the accounting for

[10] Camb. Trans. iv, p. 83, &c. [11] Ib. p. 122.

the continued dilatation of circles, and the general representation of the form of spirals, as with the explanations of the minute deviations from symmetry; as when circles become almost square, and crosses are inclined to the plane of polarization. And I believe that any one who shall follow my investigation, and imitate my experiments, will be surprised at their perfect agreement."

4. *Differential Equations of Elliptical Polarization.*—Although circular and elliptical polarization can be clearly conceived, and their existence, it would seem, irresistibly established by the phenomena, it is extremely difficult to conceive any arrangement of the particles of bodies by which such motions can mechanically be produced; and this difficulty is the greater, because some fluids and some gases impress a circular polarization upon light, in which cases we cannot imagine any definite arrangement of the particles, such as might form the mechanism requisite for the purpose. Accordingly, it does not appear that any one has been able to suggest even a plausible hypothesis on this subject. Yet, even here something has been done. Professor M'Cullagh of Dublin, has discovered that by slightly modifying the analytical expressions resulting from the common case of the propagation of light, we may obtain other expressions which would give rise to such motions as produce circular and elliptical polarization. And though we cannot as yet assign the mechanical interpretation of the language of analysis thus generalised, this generalisation brings together and explains by

one common numerical supposition, two distinct classes of facts;—a circumstance which, in all cases, entitles an hypothesis to a very favourable consideration.

Mr. M'Cullagh's assumption consists in adding to the two equations of motion which are expressed by means of second differentials, two other terms involving third differentials in a simple and symmetrical manner. In doing this, he introduces a coefficient, of which the magnitude determines both the amount of rotation of the polarization of a ray passing along the axis, as observed and measured by M. Biot, and the ellipticity of the polarization of a ray which is oblique to the axis according to Mr. Airy's theory, of which ellipticity that philosopher also had obtained certain measures. The agreement between the two sets of measures[18] thus brought into connexion is such as very strikingly to confirm Mr. M'Cullagh's hypothesis. It appears probable, too, that the confirmation of this hypothesis involves, although in an obscure and oracular form, a confirmation of the undulatory theory, which is the starting-point of this curious speculation.

5. *Elliptical Polarization of Metals.*—The effect of metals upon the light which they reflect, was known from the first to be different from that which transparent bodies produce. Sir David Brewster, who has recently examined this subject very fully[19], has described the modification thus produced, as *elliptic polarization.* In employing this term, "he seems to

[18] Royal I. A. Trans. 1836. [19] Phil. Trans. 1830.

have been led," it has been observed[14], "by a desire to avoid as much as possible all reference to theory. The laws which he has obtained, however, belong to elliptically-polarized light in the sense in which the term was introduced by Fresnel." And the identity of the light produced by metallic reflection with the elliptically-polarized light of the wave theory, is placed beyond all doubt, by an observation of Professor Airy, that the rings of uniaxal crystals, produced by Fresnel's elliptically-polarized light, are exactly the same as those produced by Brewster's metallic light.

6. *Newton's Rings by Polarized Light.*—Other modifications of the phenomena of thin plates by the use of polarized light, supplied other striking confirmations of the theory. These were in one case the more remarkable, since the result was foreseen by means of a rigorous application of the conception of the vibratory motion of light, and confirmed by experiment. Professor Airy of Cambridge was led by his reasonings to see, that if Newton's rings are produced between a lens and a plate of metal by polarized light, then, up to the polarizing angle, the central spot will be black, and instantly beyond this, it will be white. In a note[15], in which he announced this, he says, "This I anticipated from Fresnel's expressions; it is confirmatory of them, and defies emission." He also predicted that when the rings were produced between two substances of very different refractive powers,

[14] Lloyd, Report on Optics, p. 372. (Brit. Assoc.)

[15] Addressed to myself, dated May 23, 1831.

the centre would twice pass from black to white and from white to black, by increasing the angle; which anticipation was fulfilled by using a diamond for the higher refraction[16].

7. *Conical Refraction.*—In the same manner, Professor Hamilton of Dublin pointed out that according to the Fresnelian doctrine of double refraction, there is a certain direction of a crystal in which a single ray of light will be refracted so as to form a *conical pencil.* For the direction of the refracted ray is determined by a plane which touches the wave surface, the rule being that the ray must pass from the centre of the surface to the point of contact; and though in general this contact gives a single point only, it so happens, from the peculiar inflected form of the wave surface, which has what is called *a cusp*, that in one particular position, the plane can touch the surface in an entire circle. Thus the general rule which assigns the path of the refracted ray, would, in this case, guide it from the centre of the surface to every point in the circumference of the circle, and thus make it a cone. This very curious and unexpected result, which Professor Hamilton thus obtained from the theory, his friend Professor Lloyd verified as an experimental fact. We may notice also, that Professor Lloyd found the light of the conical pencil to be polarized according to a law of an unusual kind; but one which was easily seen to be in complete accordance with the theory.

8. *Fringes of Shadows.*—The phenomena of the

[16] Camb. Trans. vol. ii. p. 409.

fringes of shadows of small holes and groups of holes, which had been the subject of experiment by Fraunhofer, were at a later period carefully observed in a vast variety of cases by M. Schwerd of Spires, and published in a separate work[17], *Beugungserscheinungen*, (Phenomena of Inflection,) 1836. In this Treatise, the author has with great industry and skill calculated the integrals which, as we have seen, are requisite in order to trace the consequences of the theory; and the accordance which he finds between these and the varied and brilliant results of observation is throughout exact. "I shall," says he, in the preface[18], "prove by the present Treatise, that all inflection-phenomena, through openings of any form, size, and arrangement, are not only explained by the undulation-theory, but that they can be represented by analytical expressions, determining the intensity of the light in any point whatever." And he justly adds, that the undulation-theory accounts for the phenomena of light, as completely as the theory of gravitation does for the facts of the solar system.

9. *Objections to the Theory.*—We have hitherto mentioned only cases in which the undulatory theory was either entirely successful in explaining the facts, or at least hypothetically consistent with them and with itself. But other objections were started, and some difficulties were long considered as very em-

[17] Die Beugungs-erscheinungen, aus dem Fundamental-gesetz der Undulations Theorie analytisch entwickelt und in Bildern dargestellt, von. F. M. Schwerd. Mannheim, 1835.

[18] Dated Speyer, Aug. 1835.

barrassing. Objections were made to the theory by some English experimenters, as Mr. Potter, Mr. Barton, and others. These appeared in scientific journals, and were afterwards answered in similar publications. The objections depended partly on the measure of the *intensity* of light in the different points of the phenomena, (a datum which it is very difficult to obtain with accuracy by experiment;) and partly on misconceptions of the theory; and I believe there are none of them which would now be insisted on.

We may mention, also, another difficulty, which it was the habit of the opponents of the theory to urge as a reproach against it, long after it had been satisfactorily explained: I mean the *half-undulation* which Young and Fresnel had found it necessary, in some cases, to assume as gained or lost by one of the rays. Though they and their followers could not analyze the mechanism of reflection with sufficient exactness to trace out all the circumstances, it was not difficult to see, upon Fresnel's principles, that reflection from the interior and exterior surface of glass must be of opposite kinds, which might be expressed by supposing one of these rays to lose half an undulation. And thus there came into view a justification of the step which had originally been taken upon empirical grounds alone.

10. *Dispersion, on the Undulatory Theory.*—A difficulty of another kind occasioned a more serious and protracted embarrassment to the cultivators of this theory. This was the apparent impossibility of

accounting, on the theory, for the prismatic dispersion of colour. For it had been shown by Newton that the amount of refraction is different for every colour; and the amount of refraction depends on the velocity with which light is propagated. Yet the theory suggested no reason why the velocity should be different for different colours: for, by mathematical calculation, vibrations of all degrees of rapidity (in which alone colours differ) are propagated with the same speed. Nor does analogy lead us to expect this variety. There is no such difference between quick and slow waves of air. The sounds of the deepest and the highest bells of a peal are heard at any distance in the same order. Here, therefore, the theory was at fault.

But this defect was far from being a fatal one. For though the theory did not explain, it did not contradict, dispersion. The suppositions on which the calculations had been conducted, and the analogy of sound, were obviously in no small degree precarious. The velocity of propagation might differ for different rates of undulation, in virtue of many causes which would not affect the general theoretical results.

Many such hypothetical causes were suggested by various eminent mathematicians, as solutions of this conspicuous difficulty. But without dwelling upon these conjectures, it may suffice to notice that hypothesis upon which the attention of mathematicians was soon concentrated. This was the *hypothesis of finite intervals* between the particles of the ether. The length of one of those undulations which pro-

duce light, is a very small quantity, its mean value being 1-50,000th of an inch; but in the previous investigations of the consequences of the theory, it had been assumed that the distance from each other, of the particles of the ether, which, by their attractions or repulsions, caused the undulations to be propagated, is indefinitely less than this small quantity;—so that its amount might be neglected in the cases in which the length of the undulation was one of the quantities which determined the result. But this assumption was made arbitrarily, as a step of simplification, and because it was imagined that, in this way, a nearer approach was made to the case of a continuous fluid ether, which the supposition of distinct particles imperfectly represented. It was still free for mathematicians to proceed upon the opposite assumption, of particles of which the distances were finite, either as a mathematical basis of calculation, or as a physical hypothesis; and it remained to be seen if, when this was done, the velocity of light would still be the same for different lengths of undulation, that is, for different colours. M. Cauchy, calculating, upon the most general principles, the motion of such a collection of particles as would form an elastic medium, obtained results which included the new extension of the previous hypothesis. Professor Powell, of Oxford, applied himself to reduce to calculation, and to compare with experiment, the result of these researches. And it appeared that, on M. Cauchy's principles, a variation in the velocity of light is produced by a

variation in the length of the wave, provided that the interval between the molecules of the ether bears a sensible ratio to the length of an undulation[19]. Professor Powell obtained also, from the general expressions, a formula expressing the relation between the refractive index of a ray and the length of a wave, or the colour of light[20]. It then became his task to ascertain whether this relation obtained experimentally; and he found a very close agreement between the numbers which resulted from the formula and those observed by Fraunhofer, for ten different kinds of media, namely, certain glasses and fluids[21]. To these he afterwards added ten other cases of crystals observed by Rudberg[22]. Mr. Kelland, of Cambridge, also calculated, in a manner somewhat different, the results of the same hypothesis of finite intervals[23], and obtaining formulæ not exactly the same as Professor Powell, found also an agreement between these and Fraunhofer's observations.

It may be observed, that the refractive indices observed and employed in these comparisons, were not those determined by the colour of the ray, which is not capable of exact identification, but those more accurate measures which Fraunhofer was enabled to make, in consequence of having detected in the spectrum the black lines which he called B, C, D, E, F, G, H. The agreement between the theoretical

[19] Phil. Mag. vol. vi. p. 266. [20] Ib. vol. vii., 1835, p. 266.
[21] Phil. Trans. 1835, p. 249.
[22] Ib. 1836, p. 17. [23] Camb. Trans. vol. vi. p. 153.

formulæ and the observed numbers is remarkable, throughout all the series of comparisons of which we have spoken. Yet we must at present hesitate to pronounce upon the hypothesis of finite intervals, as proved by these calculations; for though this hypothesis has given results agreeing so closely with experiment, it is not yet clear that other hypotheses may not produce an equal agreement. By the nature of the case, there must be a certain gradation and continuity in the succession of colours in the spectrum, and hence, any supposition which will account for the general fact of the whole dispersion, may possibly account for the amount of the intermediate dispersions, because these must be interpolations between the extremes. The result of this hypothetical calculation, however, shows very satisfactorily that there is not, in the fact of dispersion, anything which is at all formidable to the undulatory theory.

11. *Conclusion.*—There are several other of the more recondite points of the theory which may be considered as, at present, too undecided to allow us to speak historically of the discussions which they have occasioned[24]. For example, it was conceived, for some time, that the vibrations of polarized light are perpendicular to the plane of polarization. But this assumption was not an essential part of the theory; and all the phenomena would equally allow us

[24] For an account of these, see Professor Lloyd's Report on Physical Optics.

to suppose the vibrations to be in the polarization-plane; the main requisite being, that light polarized in planes at right angles to each other, should also have the vibrations at right angles. Accordingly, for some time, this point was left undecided by Young and Fresnel, and, more recently, some mathematicians have come to the opinion that ether vibrates in the plane of polarization. The theory of transverse vibrations is equally stable, whichever supposition may be finally confirmed.

We may speak, in the same manner, of the suppositions which, from the time of Young and Fresnel, the cultivators of this theory have been led to make respecting the mechanical constitution of the ether, and the forces by which transverse vibrations are produced. It was natural that various difficulties should arise upon such points, for transverse vibrations had not previously been made the subject of mechanical calculation, and the forces which occasion them must act in a different manner from those which were previously contemplated. Still, we may venture to say, without entering into these discussions, that it has appeared, from all the mathematical reasonings which have been pursued, that there is not, in the conception of transverse vibrations, anything inconsistent either with the principles of mechanics, or with the best general views we can form of the forces by which the universe is held together.

I willingly speak as briefly as the nature of my undertaking allows, of those points of the undulatory

theory which are still under deliberation among mathematicians. With respect to these, an intimate acquaintance with mathematics and physics is necessary to enable any one to understand the steps which are made from day to day; and still higher philosophical qualifications would be requisite in order to pronounce a judgment upon them. I shall, therefore, conclude this survey by remarking the highly promising condition of this great department of science, in respect to the character of its cultivators. Nothing less than profound thought and great mathematical skill can enable any one to deal with this theory, in any way likely to promote the interests of science. But there appears, in the horizon of the scientific world, a considerable class of young mathematicians, who are already bringing to these investigations the requisite talents and zeal; and who, having acquired their knowledge of it since the time when its acceptation was doubtful, possess, without effort, that singleness and decision of view as to its fundamental doctrines, which it is difficult for those to attain whose minds have had to go through the hesitation, struggle, and balance of the epoch of the establishment of the theory. In their hands, it is reasonable to suppose the analytical mechanics of light will be improved as much as the analytical mechanics of the solar system was by the successors of Newton. We have already had to notice many of this younger race of undulationists. For besides MM. Cauchy, Poisson, and Ampère, M. Lainé has been more recently

following these researches in France[25]. In Belgium, M. Quetelet has given great attention to them; and, in our own country, Sir William Hamilton, and Professor Lloyd, of Dublin, have been followed by Mr. M'Cullagh. Professor Powell, of Oxford, has continued his researches with unremitting industry; and, at Cambridge, Professor Airy, who did much for the establishment and diffusion of the theory before he was removed to the post of Astronomer Royal, at Greenwich, has had the satisfaction to see his labours continued by others, even to the most recent time; for Mr. Kelland[26], whom we have already mentioned, and Mr. Smith[27], the two persons who, in 1834 and 1836, received the highest mathematical honours which that university can bestow, have both of them published investigations respecting the undulatory theory. We may be permitted to add, as a reflection obviously suggested by these facts, that the cause of the progress of science is incalculably benefited by the existence of a body of men, trained and stimulated to the study of the higher mathematics, such as exists in the British universities, who are thus prepared, when an abstruse and sublime theory comes before the world with all the characters of

[25] Prof. Lloyd's Report, p. 392.

[26] On the Dispersion of Light, as explained by the hypothesis Finite Intervals. Camb. Trans. vol. vi. p. 153.

[27] Investigation of the Equation to Fresnel's Wave Surface, ib. p. 85. See also, in the same volume, Mathematical Considerations on the Problem of the Rainbow, showing it to belong to Physical Optics, by R. Potter, Esq., of Queen's College.

truth, to appreciate its evidence, to take steady hold of its principles, to pursue its calculations, and thus to convert into a portion of the permanent treasure and inheritance of the civilized world, discoveries which might otherwise expire with the great geniuses who produced them, and be lost for ages, as, in former times, great scientific discoveries have sometimes been.

The reader who is acquainted with the history of recent optical discovery, will see that we have omitted much which has justly excited admiration; as, for example, the phenomena produced by glass under heat or pressure, noticed by Lobeck, Biot, and Brewster, and many most curious properties of particular minerals. We have omitted, too, all notice of the phenomena and laws of the absorption of light, which hitherto stand unconnected with the theory. But in this we have not materially deviated from our main design; for our end, in what we have done, has been to trace the advances of optics towards perfection as a theory; and this task we have now nearly executed as far as our abilities allow.

We have been desirous of showing that the *type* of this progress in the histories of the two great sciences, Physical Astronomy and Physical Optics, is the same. In both we have many *Laws of Phenomena* detected and accumulated by acute and inventive men; we have *Preludial* guesses which touch the true theory, but which remain for a time imperfect, undeveloped, unconfirmed: finally, we have the *Epoch* when this true theory, clearly apprehended by

great philosophical geniuses, is recommended by its fully explaining what it was first meant to explain, and confirmed by its explaining what it was *not* meant to explain. We have then its *Progress*, struggling for a little while with adverse prepossessions and difficulties; finally overcoming all these, and moving onwards, while its triumphal procession is joined by all the younger and more vigorous men of science.

It would, perhaps, be too fanciful to attempt to establish a parallelism between the prominent persons who figure in these two histories. If we were to do this, we must consider Huyghens and Hooke as standing in the place of Copernicus, since, like him, they announced the true theory, but left it to a future age to give it developement and mechanical confirmation; Malus and Brewster, grouping them together, correspond to Tycho Brahe and Kepler, laborious in accumulating observations, inventive and happy in discovering laws of phenomena; and Young and Fresnel combined, make up the Newton of optical science.

BOOK X.

SECONDARY MECHANICAL SCIENCES.

(CONTINUED.)

HISTORY

OF

THERMOTICS AND ATMOLOGY.

Et primum faciunt ignem se vortere in auras
Aëris; hinc imbrem gigni terramque creari
Ex imbri; retroque a terrâ cuncta revorti,
Humorem primum, post aëra deinde calorem;
Nec cessare hæc inter se mutare, meare,
De cœlo ad terram de terrâ ad sidera mundi.

LUCRETIUS, i. 783.

Water, and air, and fire, alternate run
Their endless circle, multiform, yet one.
For, moulded by the fervour's latent beams,
Solids flow loose, and fluids flash to steams,
And elemental flame, with secret force,
Pursues through earth, air, sky, its stated course.

INTRODUCTION.

Of Thermotics and Atmology.

I EMPLOY the term *Thermotics*, to include all the doctrines respecting heat, which have hitherto been established on proper scientific grounds. Our survey of the history of this branch of science must be more rapid and less detailed than it has been in those subjects of which we have hitherto treated: for our knowledge is, in this case, more vague and uncertain than in the others, and has made less progress towards a general and certain theory. Still, the narrative is too important and too instructive to be passed over.

The distinction of Formal Thermotics and Physical Thermotics,—of the discovery of the mere laws of phenomena, and the discovery of their causes,—is applicable here, as in other departments of our knowledge. But we cannot exhibit, in any prominent manner, the latter division of the science now before us; since no general theory of heat has yet been propounded, which affords the means of calculating the circumstances of the phenomena of conduction, radiation, expansion, and change of solid, liquid, and gaseous form. But on each of these subjects there have been proposed, and extensively assented to, certain general views, each of which

explains its appropriate class of phenomena; and, in some cases, these principles have been clothed in precise and mathematical conditions, and thus made bases of calculation.

These principles, thus possessing a generality of a limited kind, connecting several observed laws of phenomena, but yet not connecting all the observed classes of facts which relate to heat, will require our separate attention. They may be described as the Doctrine of Conduction, the Doctrine of Radiation, the Doctrine of Specific Heat, and the Doctrine of Latent Heat; and these, and similar doctrines respecting heat, make up the science which we may call *Thermotics proper*.

But besides these collections of principles which regard heat by itself, the relations of heat and moisture give rise to another extensive and important collection of laws and principles, which I shall treat of in connexion with Thermotics, and shall term *Atmology*, borrowing the term from the Greek word (ἄτμος,) which signifies *vapour*. The atmosphere was so named by the Greeks, as being a sphere of vapour; and, undoubtedly, the most general and important of the phenomena which take place in the air, by which the earth is surrounded, are those in which water, of one *consistence* or other (ice, water, or steam), is concerned. The knowledge which relates to what takes place in the atmosphere has been called *Meteorology*, in its collective form: but such knowledge is, in fact, composed of parts of many different sciences. And it is useful for our

purpose to consider separately those portions of Meteorology which have reference to the laws of aqueous vapour, and these we may include under the term Atmology.

The instruments which have been invented for the purpose of measuring the moisture of the air, that is, the quantity of vapour which exists in it, have been termed Hygrometers; and the doctrines on which these instruments depend, and to which they lead, have been called *Hygrometry;* but this term has not been used in quite so extensive a sense as that which we intend to affix to Atmology.

In treating of Thermotics, we shall first describe the earlier progress of men's views concerning Conduction, Radiation, and the like, and shall then speak of the more recent corrections and extensions, by which they have been brought nearer to theoretical generality.

THERMOTICS PROPER.

CHAPTER I.

The Doctrines of Conduction and Radiation.

Sect. 1.—*Introduction of the Doctrine of Conduction.*

By *conduction* is meant the propagation of heat from one part to another of a continuous body; or from one body to another in contact with it; as when one end of a poker stuck in the fire heats the other end, or when this end heats the hand which takes hold of it. By *radiation* is meant the diffusion of heat from the surface of a body to points not in contact. It is clear in both these cases, that, in proportion as the hot portion is hotter, it produces a greater effect in warming the cooler portion; that is, it *communicates more heat* to it, if *heat* be the abstraction of which this effect is the measure. The simplest rule which can be proposed is, that the heat thus communicated in a given instant is proportional to the excess of the heat of the hot body over that of the contiguous bodies; there are no obvious phenomena which contradict the supposition that this is the true law; and it was thence assumed by Newton as the true law for radiation, and by other writers for conduction. This assumption was confirmed approximately, and terwards corrected,

for the case of radiation; in its application to conduction, it has been made the basis of calculation up to the present time. We may observe that this statement takes for granted that we have attained to a measure of heat (or of *temperature*, as heat thus measured is termed,) corresponding to the law thus assumed; and, in fact, as we shall have occasion to explain in speaking of the *measures* of sensible qualities, the thermometrical scale of heat according to the expansion of liquids, (which is the measure of temperature here adopted,) was constructed with a reference to Newton's law of radiation of heat; and thus the law is necessarily consistent with the scale.

In any case in which the parts of a body are unequally hot, the temperature will vary *continuously* in passing from one part of the body to another; thus, a long bar of iron, of which one end is kept red-hot, will exhibit a *gradual* diminution of temperature at successive points, proceeding to the other end. The law of temperature of the parts of such a bar might be expressed by the ordinates of a *curve* which should run alongside the bar. And, in order to trace mathematically the consequences of the assumed law, some of those processes would be necessary, by which mathematicians are enabled to deal with the properties of curves, as the method of infinitesimals, or the differential calculus; and the truth or falsehood of the law would be determined, according to the usual rules of inductive science, by a comparison of results so deduced from the principle, with the observed phenomena.

It was easily perceived that this comparison was the task which physical inquirers had to perform; but the execution of it was delayed for some time; partly, perhaps, because the mathematical process presented some difficulties. Even in a case so simple as that above mentioned, of a linear bar with a stationary temperature at one end, partial differentials entered; for there were three variable quantities, the time, as well as the place and temperature. And at first, another scruple occurred to Biot when, about 1804, he undertook this problem[1]. "A difficulty," says Laplace[2], in 1809, "presents itself, which has not yet been solved. The quantities of heat received and communicated in an instant (by any point of the bar) must be infinitely small quantities of the same order as the excess of the heat of a slice of the body over that of the contiguous slice; therefore the *excess* of the heat received by any slice over the heat communicated, is an infinitely small quantity of the second order; and the accumulation in a finite time (which depends on this excess) cannot be finite." I conceive that this difficulty arises entirely from an arbitrary and unnecessary assumption concerning the relations of the infinitesimal parts of the body. Laplace resolved the difficulty by further reasoning, founded upon the same assumption which occasioned it; but Fourier, who was the most distinguished of the cultivators of this mathematical doctrine of conduc-

[1] Biot, Traité de Phys. iv. p. 669.

[2] Laplace, Mém. Inst. for 1809, p. 332.

tion, follows a course of reasoning in which the difficulty does not present itself. Indeed it is stated by Laplace, in the Memoir above quoted[3], that Fourier had already obtained the true fundamental equations by views of his own.

The remaining part of the history of the doctrine of conduction is principally the history of Fourier's labours. Attention having been drawn to the subject, as we have mentioned, the French Institute, in January, 1810, proposed, as their prize-question, "To give the mathematical theory of the laws of the propagation of heat, and to compare this theory with exact observations." Fourier's Memoir (the sequel of one delivered in 1807,) was sent in September, 1811; and the prize (3000 francs) adjudged to it in 1812. In consequence of the political confusion which prevailed in France, or of other causes, these important Memoirs were not published by the Academy till 1824; but extracts had been printed in the Bulletin des Sciences in 1808, and in the Annales de Chimie in 1816; and Poisson and Cauchy had consulted the manuscript itself.

It is not my purpose to give, in this place[4], an account of the analytical processes by which Fourier obtained his results. The skill displayed in these Memoirs is such as to make them an object of just admiration to mathematicians; but they consist entirely of deductions from the fundamental prin-

[3] Laplace, Mém. Inst. for 1809, p. 538.

[4] I have given an account of Fourier's mathematical results in the Report of the British Association for 1835.

ciple which I have noticed,—that the quantity of heat conducted from a hotter to a colder point is proportional to the excess of heat, modified by the *conductivity*, or conducting power of each substance. The equations which flow from this principle assume nearly the same forms as those which occur in the most general problems of hydrodynamics. Besides Fourier's solution, Laplace, Poisson, and Cauchy have also exercised their great analytical skill in the management of these formulæ. We shall briefly speak of the comparison of the results of these reasonings with experiment, and notice some other consequences to which they lead. But before we can do this, we must pay some attention to the subject of radiation.

Sect. 2.—*Introduction of the Doctrine of Radiation.*

A HOT body, as a mass of incandescent iron, emits heat, as we perceive by our senses when we approach it; and in this manner the hot body cools down. The first step in our systematic knowledge of this subject was made in the Principia. "It was in the destiny of that great work," says Fourier, "to exhibit, or at least, to indicate, the causes of the principal phenomena of the universe." Newton assumed, as we have already said, that the rate at which a body cools, that is, parts with its heat to surrounding bodies, is proportional to its heat; and on this assumption he rested the verification of his scale of temperatures. It is an easy deduction from

this law, that if times of cooling be taken in arithmetical progression, the heat will decrease in geometrical progression. Kraft, and after him Richman, tried to verify this law by direct experiments on the cooling of vessels of warm water; and from these experiments, which have since been repeated by others, it appears that for differences of temperature which do not exceed 50 degrees (boiling water being 100), this geometrical progression represents, with tolerable (but not with complete) accuracy, the process of cooling.

This principle of radiation, like that of conduction, required to be followed out by mathematical reasoning. But it required also to be corrected in the first place, for it was easily seen that the rate of cooling depended, not on the absolute temperature of the body, but on the excess of its temperature above the surrounding objects to which it communicated its heat in cooling. And philosophers were naturally led to endeavour to explain or illustrate this process by some physical notions. Lambert in 1755 published[3] an *Essay on the Force of Heat*, in which he assimilates the communication of heat to the flow of a fluid out of one vessel into another by an excess of pressure; and mathematically deduces the laws of the process on this ground. But some additional facts suggested a different view of this subject. It was found that heat is propagated by radiation according to straight lines, like light; and that it is, as light is, capable of being reflected by

[3] Act. Helvet. tom. ii, p. 172.

mirrors, and thus brought to a focus of intenser action. In this manner the radiative effect of a body could be more precisely traced. A fact, however, came under notice, which, at first sight, appeared to offer some difficulty. It appeared that cold was reflected no less than heat. A mass of ice, when its effect was concentrated on a thermometer by a system of mirrors, made the thermometer fall, just as a vessel of hot water made it rise. Was cold, then, to be supposed a real substance, no less than heat?

The solution of this and similar difficulties was given by Pierre Prevost, professor at Geneva, whose theory of radiant heat was proposed about 1790. According to this theory, heat, or *caloric*, is constantly radiating from every point of the surface of all bodies in straight lines; and it radiates the more copiously, the greater is the quantity of heat which the body contains. Hence a constant exchange of heat is going on among neighbouring bodies; and a body grows hotter or colder, accordingly as it receives more caloric than it emits, or the contrary. And thus a body is cooled by rectilinear rays from a cold body, because along these paths it sends rays of heat in greater abundance than those which return the same way. This *theory of exchanges* is simple and satisfactory, and was soon generally adopted; but we must consider it rather as the simplest mode of expressing the dependence of the communication of heat on the excess of temperature, than as a proposition of which the physical truth is clearly established.

A number of curious researches on the effect of the different kinds of surface of the heating and of the heated body, were made by Leslie and others. On these I shall not dwell; only observing, that the relative amount of this radiative and receptive energy may be expressed by numbers, for each kind of surface; and that we shall have occasion to speak of it under the term *exterior conductivity;* it is thus distinguished from *interior conductivity*, which is the relative rate at which heat is conducted in the interior of bodies. The term employed by Fourier, *conductibility* or *conducibility*, suggests expressions altogether absurd, as if the bodies could be called *conductible*, or *conducible*, with respect to heat: I have therefore ventured upon a slight alteration of the word, and have used the abstract term which analogy would suggest, if we suppose bodies to be denominated *conductive* in this respect.

Sect. 3.—*Verification of the Doctrines of Conduction and Radiation.*

THE interior and exterior conductivity of bodies are numbers, which enter as elements, or *co-efficients*, into the mathematical calculations founded on the doctrines of conduction and radiation. These co-efficients are to be determined for each case by appropriate experiments; when the experimenters had obtained these data, as well as the mathematical solutions of the problems, they could test the truth of their fundamental principles by a comparison of

the theoretical and actual results in properly-selected cases. This was done for the law of conduction in the simple cases of metallic bars heated at one end, by Biot[6], and the accordance with experiment was sufficiently close. In the more complex cases of conduction which Fourier considered, it was less easy to devise a satisfactory mode of comparison. But some rather curious relations which he demonstrated to exist among the temperatures of a ring, or *armille*, afforded a good criterion of the value of the calculations, and confirmed their correctness[7].

We may therefore presume these doctrines of radiation and conduction to be sufficiently established; and we may consider their application to any remarkable case to be a portion of the history of science. We proceed to some such applications.

Sect. 4.—*The Geological and Cosmological Application of Thermotics.*

By far the most important case to which conclusions from these doctrines have been applied, is that of the globe of the earth, and of those laws of climate to which the modifications of temperature give rise; and in this way we are led to inferences concerning other parts of the universe. If we had any means of observing these terrestrial and cosmical phenomena to a sufficient extent, they would be valuable

[6] Tr. de Phys. iv. 671.

[7] Mém. Inst. 1819, p. 192, published 1824.

facts on which we might erect our theories; and thus form part, not of the corollaries, but of the foundations of our doctrine of heat. In such a case, the laws of the propagation of heat, as discovered from experiments on smaller bodies, would serve to explain these phenomena of the universe, just as the laws of motion explain the celestial movements. But since we are almost entirely without any definite indications of the condition of the other bodies in the solar system as to heat; and since, even with regard to the earth, we know only the temperature of the parts at or very near the surface, our knowledge of the part which heat plays in the earth and the heavens must be in a great measure, not a generalization of observed facts, but a deduction from theoretical principles. Still, such knowledge, whether obtained from observation or from theory, must possess great interest and importance. The doctrines of this kind which we have to notice refer principally to the effect of the sun's heat on the earth, the laws of climate, the thermotical condition of the interior of the earth, and that of the planetary spaces.

1. *Effect of Solar Heat on the Earth.*—That the sun's heat passes into the interior of the earth in a variable manner, depending upon the succession of days and nights, summers and winters, is an obvious consequence of our first notions on this subject. The mode in which it proceeds into the interior, after descending below the surface, remained to be gathered, either from the phenomena, or from rea-

soning. Both methods were employed[8]. Saussure endeavoured to trace its course by digging, in 1785, and thus found that at the depth of about thirty-one feet, the annual variation of temperature is about 1-12th what it is at the surface. Leslie adopted a better method, sinking the bulbs of thermometers deep in the earth, while their stems appeared above the surface. In 1815, 16, and 17, he observed thus the temperatures at the depths of one, two, four, and eight feet, at Abbotshall, in Fifeshire. The results showed that the extreme annual oscillations of the temperature diminish as we descend. At the depth of one foot, the yearly range of oscillation was twenty-five degrees (Fahrenheit); at two feet it was twenty degrees; at four feet it was fifteen degrees; at eight feet, it was only nine degrees and a half. And the time at which the heat was greatest was later and later in proceeding to the lower points. At one foot, the maximum and minimum were three weeks after the solstice of summer and of winter; at two feet, they were four or five weeks; at four feet, they were two months; and at eight feet, three months. The mean temperature of all the thermometers was nearly the same. Similar results were obtained by Ott at Zurich in 1762, and by Herrenschneider at Strasburg in 1821, 2, 3[9].

These results had already been explained by Fourier's theory of conduction. He had shown[10]

[8] Leslie, art. Climate, Supp. Enc. Brit. 179.

[9] Pouillet, Météorol. t. ii. p. 643.

[10] Mém. Inst. for 1821 (published 1826), p. 162.

that when the surface of a sphere is affected by a periodical heat, certain alternations of heat travel uniformly into the interior, but that the extent of the alternation diminishes in geometrical progression in this descent. This conclusion applies to the effect of days and years on the temperature of the earth, and shows that such facts as those observed by Leslie are both exemplifications of the general circumstances of the earth, and are perfectly in accordance with the principles on which Fourier's theory rests.

2. *Climate.*—The term *climate*, which means *inclination*, was applied by the ancients to denote that inclination of the axis of the terrestrial sphere from which result the inequalities of days in different latitudes. This inequality is obviously connected also with a difference of thermotical condition. Places near the poles are colder, on the whole, than places near the equator. It was a natural object of curiosity to determine the law of this variation.

Such a determination, however, involves many difficulties, and the settlement of several preliminary points. How is the temperature of any place to be estimated? and if we reply, by its *mean* temperature, how are we to learn this mean? The answers to such questions require very multiplied observations, exact instruments, and judicious generalizations; and cannot be given here. But certain first approximations may be obtained without much difficulty; for instance, the mean temperature of any place may be taken to be the temperature of deep springs, which

is probably identical with the temperature of the soil below the reach of the annual oscillations. Proceeding on such facts, Mayer found that the mean temperature of any place was nearly proportional to the square of the cosine of the latitude. This, as a law of phenomena, has since been found to require considerable correction; and it appears that the mean temperature does not depend on the latitude alone, but on the distribution of land and water, and on other causes. Humboldt has expressed these deviations[11] by his map of *isothermal lines*, and Brewster has endeavoured to reduce them to a law by assuming two *poles of maximum cold*.

The expression which Fourier finds[12] for the distribution of heat in a homogeneous sphere, is not immediately comparable with Mayer's empirical formula, being obtained on a certain hypothesis, namely, that the equator is kept constantly at a fixed temperature. But there is still a general agreement; for, according to the theory, there is a diminution of heat in proceeding from the equator to the poles in such a case; the heat is propagated from the equator and the neighbouring parts, and radiates out from the poles into the surrounding space. And thus, in the case of the earth, the solar heat enters in the tropical parts, and constantly flows towards the polar regions, by which it is emitted into the planetary spaces.

[11] British Assoc. 1833. Forbes's Report on Meteorology, p. 215.

[12] Fourier, Mém. Inst. tom. v. p. 173.

Climate is affected by many thermotic influences, besides the conduction and radiation of the solid mass of the earth. The atmosphere, for example, produces upon terrestrial temperatures effects which it is easy to see are very great; but these it is not yet in the power of calculation to appreciate[13]; and it is clear that they depend upon other properties of air besides its power to transmit heat. We must therefore dismiss them, at least for the present.

3. *Temperature of the Interior of the Earth.*—The question of the temperature of the interior of the earth has excited great interest, in consequence of its bearing on other branches of knowledge. The various facts which have been supposed to indicate the fluidity of the central parts of the terrestrial globe, belong, in general, to geological science; but so far as they require the light of thermotical calculations in order to be rightly reasoned upon, they properly come under our notice here.

The principal problem of this kind which has been treated of is this:—If in the globe of the earth there be a certain original heat, resulting from its earlier condition, and independent of the action of the sun, to what results will this give rise? and how far do the observed temperatures of points below the surface lead us to such a supposition. It has, for instance, been asserted, that in many parts of the world the temperature, as observed in mines and other excavations, increases in descending, at the

[13] Fourier, Mém. Inst. tom. vii. p. 584.

rate of one degree (centesimal) in about forty yards. What inference does this justify?

The answer to this question was given by Fourier and by Laplace. The former mathematician had already considered the problem of the cooling of a large sphere, in his Memoirs of 1807, 1809, and 1811. These, however, lay unpublished in the archives of the Institute for many years. But in 1820, when the accumulation of observations which indicated an increase of the temperature of the earth as we descend, had drawn observation to the subject, Fourier gave, in the Bulletin of the Philomathic Society[14], a summary of his results, as far as they bore on this point. His conclusion was, that such an increase of temperature in proceeding towards the centre of the earth, can arise from nothing but the remains of a primitive heat;—that the heat which the sun's action would communicate, would, in its final and permanent state, be uniform in the same vertical line, as soon as we get beyond the influence of the superficial oscillations of which we have spoken;—and that, before the distribution of temperature reaches this limit, it will decrease, not increase, in descending. It appeared also, by the calculation, that this remaining existence of the primitive heat in the interior parts of the earth's mass, was quite consistent with the absence of all perceptible traces of it at the surface; and that the same state of things which produces an increase of one degree of heat in

[14] Bullet. des Sc. 1820, p. 58.

descending forty yards, does not make the surface a quarter of a degree hotter than it would otherwise be. Fourier was led also to some conclusions, though necessarily very vague ones, respecting the time which the earth must have taken to cool from a supposed original state of incandescence to its present condition, which time it appeared must have been very great; and respecting the extent of the future cooling of the surface, which it was shown must be insensible. Everything tended to prove that, within the period which the history of the human race embraces, no discoverable change of temperature had taken place from the progress of this central cooling. Laplace further calculated the effect[15] which any contraction of the globe of the earth by cooling would produce on the length of the day. He had already shown, by astronomical reasoning, that the day had not become shorter by 1-200th of a second, since the time of Hipparchus; and thus his inferences agreed with those of Fourier. As far as regards the smallness of the perceptible effect due to the past changes of the earth's temperature, there can be no doubt that all the curious conclusions just stated are deduced in a manner quite satisfactory, from the fact of a general increase of heat in descending below the surface of the earth; and thus our principles of speculative science have a bearing upon the history of the past changes of the universe, and give us information concerning the state of

[15] Conn. des Tems,' 1823.

things in portions of time otherwise quite out of our reach.

4. *Heat of the Planetary Spaces.*—In the same manner, this portion of science is appealed to for information concerning parts of space which are utterly inaccessible to observation. The doctrine of heat leads to conclusions concerning the temperatures of the spaces which surround the earth, and in which the planets of the solar system revolve. In his Memoir, published in 1827[16], Fourier states that he conceives it to follow from his principles, that these planetary spaces are not absolutely cold, but have a "proper heat" independent of the sun and of the planets. If there were not such a heat, the cold of the polar regions would be much more intense than it is, and the alternations of cold and warmth, arising from the influence of the sun, would be far more extreme and sudden than we find them. As the cause of this heat in the planetary spaces, he assigns the radiation of the innumerable stars which are scattered through the universe.

Fourier says[17], "We conclude from these various remarks, and principally from the mathematical examination of the question," that this is so. I am not aware that the mathematical calculation which bears peculiarly upon this point has anywhere been published. But it is worth notice, that Svanberg has been led[18] to the opinion of the same temperature in these spaces which Fourier had adopted (50 centi-

[16] Mém. Inst. tom. vii. p. 580. [17] Ib. p. 581.
[18] Berzel. Jahres Bericht, xi. p. 50.

grade below zero), by an entirely different course of reasoning, founded on the relation of the atmosphere to heat.

In speaking of this subject, I have been led to notice incomplete and perhaps doubtful applications of the mathematical doctrine of conduction and radiation. But these may at least serve to show that Thermotics is a science, which, like Mechanics, is to be established by experiments on masses capable of manipulation, but which, like that, has for its most important office the solution of geological and cosmological problems. I now return to the further progress of our thermotical knowledge.

Sect 5.—*Correction of Newton's Law of Cooling.*

In speaking of the establishment of Newton's assumption, that the temperature communicated is proportional to the excess of temperature, we stated that it was approximately verified, and afterwards corrected, (p. 468.) This correction was the result of the researches of MM. Dulong and Petit in 1817, and the researches by which they were led to the true law, are an admirable example both of laborious experiment and sagacious induction. They experimented through a very great range of temperature, (as high as two hundred and forty degrees, centigrade,) which was necessary, because the inaccuracy of Newton's law becomes considerable only at high temperatures. They removed the effect of the surrounding medium, by making their experiments in a

vacuum. They selected with great judgment the conditions of their experiments and comparisons, making one quantity vary while the others remained constant. In this manner they found, that *the quickness of cooling for a constant excess of temperature, increases in geometrical progression, when the temperature of the surrounding space increases in arithmetical progression*; whereas, according to the Newtonian law, this quickness would not have varied at all. Again, this variation being left out of the account, it appeared that *the quickness of cooling, so far as it depends on the excess of temperature of the hot body, increases as the terms of a geometrical progression diminished by a constant number, when the temperature of the hot body increases in arithmetical progression.* These two laws, with the co-efficients requisite for their application to particular substances, fully determine the conditions of cooling in a vacuum.

Starting from this determination, MM. Dulong and Petit proceeded to ascertain the effect of the medium, in which the hot body is placed, upon its rate of cooling; for this effect became a residual phenomenon, when the cooling in the vacuum was taken away. We shall not here follow this train of research; but we may briefly state, that they were led to such laws as this;—that the rapidity of cooling due to any gaseous medium in which the body is placed, is the same, so long as the excess of the body's temperature is the same, although the temperature itself vary;—that the cooling power of a gas

varies with the elasticity, according to a determined law; and other similar rules.

In reference to the process of their induction, it is worthy of notice, that they founded their reasonings upon Prevost's law of exchanges; and that, in this way, the second of their laws above stated, respecting the quickness of cooling, was a mathematical consequence of the first. It may be observed also, that their temperatures are measured by means of the air-thermometer, and that if they were estimated on another scale, the remarkable simplicity and symmetry of their results would disappear. This is a strong argument for believing such a measure of temperature to have a natural prerogative of simplicity. This belief is confirmed by other considerations; but these, depending on the laws of expansion by heat, cannot be here referred to; and we must proceed to finish our survey of the mathematical theory of heat, as founded on the phenomena of radiation and conduction, which alone have yet been traced up to general principles.

We may observe, before we quit this subject, that this correction of Newton's law will materially affect the mathematical calculations on the subject, which were made to depend on that law both by Fourier, Laplace, and Poisson. Probably, however, the general features of the results will be the same as on the old supposition. M. Libri, an Italian mathematician, has undertaken one of the problems of this kind, that of the armil, with Dulong and Petit's law

for his basis, in a Memoir read to the Institute of France in 1825, and since published at Florence[19].

Sect. 6.—Other Laws of Phenomena with respect to Radiation.

THE laws of radiation as depending upon the surface of radiating bodies, and as affecting screens of various kinds interposed between the hot body and the thermometer, were examined by several inquirers. I shall not attempt to give an account of the latter course of research, and of the different laws which luminous and non-luminous heat have been found to follow in reference to bodies, whether transparent or opaque, which intercept them. But there are two or three laws of the phenomena, on the subject of the effects of the surfaces of bodies, which are important.

1. In the first place, the powers of bodies to emit and to absorb heat, as far as depends upon their surface, appear to be in the same proportion. If we blacken the surface of a canister of hot water, it radiates heat more copiously, and in the same measure, it is more readily heated by radiation.

2. In the next place, as the radiative power increases, the power of reflection diminishes, and the contrary. A bright metal vessel reflects much heat; on this very account it does not emit much; and hence a hot fluid which such a vessel contains, remains hot longer than it does in an unpolished case.

[19] Mém. de Math. et de Phys. 1829.

3. The heat is emitted from every point of the surface of a hot body in all directions; but by no means in all directions with equal intensity. The intensity of the heating ray is as the sine of the angle which it makes with the surface.

The last law is entirely, the two former in a great measure, due to the researches of Leslie, whose "Experimental Inquiry into the Nature and Propagation of Heat," published in 1804, contains a great number of curious and striking results and speculations. The laws now just stated bear, in a very important manner, upon the formation of the theory; and we must now proceed to consider what appears to have been done in this respect; taking into account, it must still be borne in mind, only the phenomena of conduction and radiation.

Sect. 7.—Fourier's Theory of Radiant Heat.

THE above laws of phenomena being established, it was natural that philosophers should seek to acquire some conception of the physical action by which they might account, both for these laws, and for the general fundamental facts of thermotics; as, for instance, the fact that all bodies placed in an inclosed space assume, in time, the temperature of the inclosure. Fourier's explanation of this class of phenomena must be considered as happy and successful; for he has shown that the supposition, to which we are led by the most simple and general of the facts, will explain, moreover, the less obvious laws. It is an ob-

vious and general fact, that bodies which are included in the same space tend to acquire the same temperature. And this identity of temperature of neighbouring bodies requires an hypothesis, which, it is found, also accounts for the law of the sine in radiation.

This hypothesis is, that the radiation takes place, not from the surface alone of the hot body, but from all particles situated within a certain small depth of the surface. It is easy to see[80] that, on this supposition, a ray emitted obliquely from an internal particle, will be less intense than one sent forth perpendicular to the surface, because the former will be intercepted in a greater degree, having a greater length of path within the body; and Fourier shows, that whatever be the law of this intercepting power, the result will be, that the radiative intensity is as the sine of the angle made by the ray with the surface.

But this law is, as I have said, likewise necessary, in order that neighbouring bodies may tend to assume the same temperature: for instance, in order that a small particle placed within a spherical shell, should finally assume the temperature of the shell. If the law of the sines did not obtain, the final temperature of such a particle would depend upon its place in the inclosure[81]: and within a shell of ice we should have, at certain points, the temperature of boiling water and of melting iron.

This proposition may at first appear strange and

[80] Mém. Inst. t. v. 1821, p. 204. [81] An. Chim. iv. 1817, p. 129.

unlikely; but it may be shown to be a necessary consequence of the assumed principle, by very simple reasoning, which I shall give in a general form in a note[22].

This reasoning is capable of being presented in a manner quite satisfactory, by the use of mathematical symbols, and proves that Leslie's law of the sines is rigorously and mathematically true on Fourier's hypothesis. And thus Fourier's theory of *molecular extra-radiation* acquires great consistency.

[22] The following reasoning may shew the connexion of the law of the sines with the general principle of ultimate identity of neighbouring temperatures. The equilibrium and identity of temperature between an including shell and an included body, cannot obtain upon the whole, except it obtain between each pair of parts of the two surfaces of the body and of the shell; that is, any part of the one surface, in its exchanges with any part of the other surface, must give and receive the same quantity of heat. Now the quantity exchanged, so far as it depends on the receiving surface, will, by geometry, be proportional to the sine of the obliquity of that surface: and as, in the exchanges, each may be considered as receiving, the quantity transferred must be proportional to the sines of the two obliquities; that is, to that of the giving as well as of the receiving surface.

Nor is this conclusion disturbed by the consideration, that all the rays of heat which fall upon a surface are not absorbed, some being reflected according to the nature of the surface. For, by the other above-mentioned laws of phenomena, we know that, in the same measure in which the surface loses the power of admitting, it loses the power of emitting, heat; and the superficial parts gain, by absorbing their own radiation, as much as they lose by not absorbing the incident heat; so that the result of the preceding reasoning remains unaltered.

Sect. 8.—*Discovery of the Polarization of Heat.*

THE laws of which the discovery is stated in the preceding Sections of this Chapter, and the explanations given of them by the theories of conduction and radiation, all tended to make the conception of a material heat, or caloric, communicated by an actual flow and emission, familiar to men's minds; and, till lately, had led the greater part of thermotical philosophers to entertain such a view, as the most probable opinion concerning the nature of heat. But some steps have recently been made in thermotics, which appear to be likely to overturn this belief, and to make the doctrine of emission as untenable with regard to heat, as it had before been found to be with regard to light. I speak of the discovery of the polarization of heat. It being ascertained that rays of heat are polarized in the same manner as rays of light, we cannot retain the doctrine that heat radiates by the emanation of material particles, without supposing those particles of caloric to have poles; an hypothesis which probably no one would embrace; for, besides that its ill fortune in the case of light must deter speculators from it, the intimate connexion of heat and light would hardly allow us to suppose polarization in the two cases to be produced by two different kinds of machinery.

But, without here tracing further the influence which a knowledge of the polarization of heat must exercise upon the formation of our theories, we must

briefly notice this important discovery, as a law of phenomena.

The analogies and connexions between light and heat are so strong, that when the polarization of light had been discovered, men were naturally led to endeavour to ascertain whether heat possessed any corresponding property. But partly from the difficulty of obtaining any considerable effect of heat separated from light, and partly from the want of a thermometrical apparatus sufficiently delicate, these attempts led, for some time, to no decisive result. M. Berard took up the subject in 1813. He used Malus's apparatus, and conceived that he found heat to be polarized by reflection at the surface of glass, in the same manner as light, and with the same circumstances[23]. But when Professor Powell, of Oxford, a few years later (1830), repeated these experiments with a similar apparatus, he found[24] that though the heat which is conveyed along with light is, of course, polarizable, "simple radiant heat," as he terms it, did not offer the smallest difference in the two rectangular azimuths of the second glass, and thus showed no trace of polarization.

Thus, with the old thermometers, the point remained doubtful. But soon after this time, MM. Melloni and Nobili invented an apparatus, depending on certain galvanic laws, of which we shall have to speak hereafter, which they called a *thermomultiplier;*

[23] Ann. Chim. March, 1813.

[24] Edin. Journ. of Science, 1830, vol. ii. p. 303.

and which was much more sensitive to changes of temperature than any previously-known instrument. As soon as this instrument became known, Professor Forbes, of Edinburgh, eagerly set to work (in 1834) to use it, for the purpose of ascertaining, among other important points of thermotics, the polarization of heat. Instead of attempting to polarize heat by reflection, he employed the tourmaline, a crystal commonly used for polarizing light. He found that there was a portion of the heat undoubtedly polarized by the tourmaline[85], that is, that a portion of the heat which passed through two tourmalines, when they were in parallel positions, was intercepted when their axes were crossed. But he afterwards employed bundles of plates of mica, placed at the polarizing angle; and found that, with this apparatus, the results were much more distinctly marked. In this case, he found that, with non-luminous heat, and even with water below the boiling-point, the polarizing effect was evident. He also ascertained that the same substance, mica, when polarized heat is transmitted through it in a proper position, produces the effect which, in the earlier researches on polarized light, was called *depolarization:* and which here, in fact, does really show itself by a partial destruction of the differences which polarization establishes. This important discovery was soon confirmed by Melloni. Some attempts were made to suggest other causes as those by which

[85] Phil. Mag. 1835, vol. vi. p. 209. Ib. vol. vii. p. 349.

the observed differences were produced; but Professor Forbes easily showed that these were inadmissible; and the property of a difference of *sides*, which at first appeared so strange when ascribed to the rays of light, also belongs, it seems to be proved, to the rays of heat.

We may add that the property of *refraction* was also found by MM. Melloni and Forbes to belong to the rays of heat; so that some of the main facts on which the theory of light is and must be founded, belong also to heat.

But before we can consider ourselves as in a condition to turn our attention to the theory of heat, there are many other laws of phenomena, which must enter into such a theory; besides the laws of conduction and radiation: some of these other laws we shall now proceed to speak of.

CHAPTER II.

THE LAWS OF CHANGES OCCASIONED BY HEAT.

Sect. 1.—*The Law of Expansion of Gases.—Dalton and Gay-Lussac.*

THE expansion of bodies by heat was one of the first examined facts of thermotics, and was the more carefully studied, because this property was used for the measure of heat. The measurement of secondary qualities is a subject which may be considered as belonging to the philosophy of science, and if we should hereafter speak of it, we shall notice the difficulties occasioned by the different laws of dilatation which different substances follow as the temperature advances; or, as it has been termed, their different thermometrical *march*. Various attempts have been made to correct these "marches" by laws, as for instance, Dalton's conjecture that water and mercury both expand as the square of the temperature from the point of greatest contraction, the temperature being measured so as to favour such a result. None of the rules thus laid down have been of any value in leading to general laws, except one concerning the expansion of gases, which is true of all gaseous fluids alike; namely, that *for equal increments of temperature they*

expand by the same fraction of their own bulk; which fraction is *three-eighths* in proceeding from freezing to boiling water. This law was discovered by Dalton and Gay-Lussac independently of each other[1]; and is usually called by both their names, the law of Dalton and Gay-Lussac. The latter says[2], "The experiments which I have described, and which have been made with great care, prove incontestably that oxygen, hydrogen, azotic, nitrous acid, ammoniacal, muriatic acid, sulphurous acid, carbonic acid, gases, expand equally by equal increments of heat." "Therefore," he adds with a proper inductive generalization, "the result does not depend upon physical properties, and I collect that *all gases expand equally by heat.*" He then extends this to vapours, as ether. This must be one of the most important foundation-stones of any sound theory of heat.

We have already seen that the opinion that the air-thermometer is a true measure of heat, is strongly countenanced by the symmetry which, by using it, we introduce into the laws of radiation. It now appears that this result is independent of any peculiar properties in the air employed; and thus this measure has an additional character of generality and simplicity which make it highly probable that it is the true standard. This opinion is further supported by the attempts to include such facts in a theory, but before we can treat of such theories, we must speak of some other doctrines which have been introduced.

[1] Manch. Mem. vol. v. 1802; and Ann. Chim. xliii. p. 137.
[2] p. 272.

Sect. 2.—Specific Heat.—Change of Consistence.

In the attempts to obtain measures of heat, it was found that bodies had different capacities for heat; for the same quantity of heat, however measured, would raise, in different degrees, the temperature of different substances. The notion of different capacities for heat was thus introduced, and each body was thus assumed to have a specific capacity for heat, or, as it was often expressed, a *specific heat*, according to the quantity of heat which it required to raise it through a given scale of heat[3].

It was found, also, that the capacity of the same substance was different in the same substance at different temperatures. It appears from experiments of MM. Dulong and Petit, that, in general, the capacity of liquids and solids increases as we ascend in the scale of temperature.

But one of the most important thermotic facts is, that by the contraction of any mass, its temperature is increased. This is peculiarly observable in gases, as for example, common air. The amount of the increase of temperature by condensation, or of the cold produced by rarefaction, is an important datum, determining the velocity of sound, as we have already seen, and affecting many points of meteorology. The co-efficient which enters the calculation in the former case depends on the ratio of two spe-

[3] See Crawfurd on Heat; for the History of Specific Heat.

cific heats of air under different conditions; one belonging to it when the pressure is constant by which the air is contained; the other, when it is contained in a constant space.

A leading fact, also, with regard to the operation of heat on bodies is, that it changes their *form*, as it is often called, that is, their condition as solid, liquid, or air. Since the term "form" is employed in too many and various senses to be immediately understood when it is intended to convey this peculiar meaning, I shall use, instead of it, the term *consistence*, and shall hope to be excused, even when I apply this word to gases, though I must acknowledge such phraseology to be unusual. Thus the change of consistency would be *solutive* when solids become liquid or liquids gaseous, and such changes must be fundamental facts of our thermotical theories. We still want many of the laws which belong to this change; but one of them, of great importance, has been discovered, and to that we must now proceed.

Sect. 3.—*The Doctrine of Latent Heat.*

THE Doctrine of Latent Heat is this: that in the change of a body from the solid to the liquid, or from the liquid to the aërial form, heat is communicated to the body which is not indicated by the thermometer; or, as it may be expressed in accordance with the terms just explained, that in the solutive

changes of consistence, sensible heat is absorbed, and becomes latent. This[4] was observed by Deluc in 1755 or 6. It was discovered nearly about the same time by Dr. Black of Edinburgh, who was unacquainted with what Deluc had done, and who taught it in his chemical lectures as early as the year 1757 or 8. Wilcke also published this doctrine in the Swedish Transactions[5].

That snow requires a great quantity of heat to melt it; that water requires a great quantity of heat to convert it into steam; and that this heat is not indicated by a rise in the thermometer, are facts which it is not difficult to observe; but to separate these from all extraneous conditions, to group the cases together, and to seize upon the general law by which they are connected, was an effort of inductive insight, which has been considered, and deservedly, as one of the most striking events in the modern history of physics. Of this step the principal merit appears to belong to Black.

The consequences of this principle are very important, for upon it is founded the whole doctrine of evaporation; besides which, the principle of latent heat has other applications. But the relations of aqueous vapour to air are so important, and have been so long a subject of speculation, that we may with advantage dwell a little upon them. The part of science in which this is done may be called, as we have said, Atmology; and to that division of thermotics the following chapters belong.

[4] Crawfurd, p. 71.

[5] Acta Suecica, 1772, p. 97.

ATMOLOGY.

CHAPTER III.

The Relation of Vapour and Air.

Sect. 1.—*Prelude to Dalton's Doctrine of Evaporation.*

Visible clouds, smoke, distillation, gave the idea of vapour; vapour was at first conceived to be identical with air, as by Bacon[1]. It was easily collected, that by heat water might be converted into vapour. It was thought that air was thus produced in the instrument called the æolipile, in which a powerful blast is caused by a boiling fluid; but Wolf showed that the fluid was not converted into air, by using camphorated spirit of wine, and condensing the vapour after it had been formed. We need not enumerate the opinions (if very vague hypotheses may be so termed,) of Descartes, Dechales, Borelli[2]. The latter accounted for the rising of vapour by supposing it a mixture of fire and water; and thus, fire being much lighter than air, the mixture also was light. Boyle endeavoured to show that vapours do not permanently float *in vacuo*. He compared the

[1] Bacon's Hist. Nat. Cent. i. p. 27.

[2] They may be seen in Fischer, Geschichte der Physik, vol. ii. p. 175.

mixture of vapour and air to that of salt and water. He found that the pressure of the atmosphere affected the heat of boiling water; a very important fact. Boyle proved this by means of the air-pump; and he and his friends were much surprised to find that when air was removed, water only just warm boiled violently. Huyghens mentions an experiment of the same kind made by Papin about 1673.

The ascent of vapour was explained in various ways in succession, according to the changes which physical science underwent. It was first distinctly treated of, at a period when hydrostatics had accounted for many phenomena; and attempts were naturally made to reduce this fact to hydrostatical principles. An obvious hypothesis, which brought it under the dominion of these principles, was, to suppose that the water, when converted into vapour, was divided into small hollow globules;—thin pellicles including air or heat. Halley gave such an explanation of evaporation; Leibnitz calculated the dimensions of these little bubbles; Derham managed (as he supposed) to examine them with a magnifying glass; Wolf also examined and calculated on the same subject. It is curious to see so much confidence in so lame a theory; for if water became hollow globules in order to rise as vapour, we require, in order to explain the formation of these globules, new laws of nature, which are not even hinted at by the supporters of the doctrine, though they must be far more complex than the hydrostatical law by which a hollow sphere floats.

Newton's opinion was hardly more satisfactory; he[s] explained evaporation by the repulsive power of heat; the parts of vapours, according to him, being small, are easily affected by this force, and thus become lighter than the atmosphere.

Muschenbroek still adhered to the theory of globules, as the explanation of evaporation; but he was manifestly discontented with it; and reasonably apprehended that the pressure of the air would destroy the frail texture of these bubbles. He called to his aid a rotation of the globules; (which Descartes also had assumed;) and, not satisfied with this, threw himself on electrical action as a reserve. Electricity, indeed, was now in favour, as hydrostatics had been before; and was naturally called in, in all cases of difficulty. Desaguliers, also, uses this agent to account for the ascent of vapour, introducing it into a kind of sexual system of clouds; according to him, the male fire (heat) does a part, and the female fire (electricity) performs the rest. These are speculations of small merit and no value.

In the mean time, Chemistry made great progress in the estimation of philosophers, and had its turn in the explanation of the important facts of evaporation. Bouillet who, in 1742, placed the particles of water in the interstices of those of air, may be considered as approaching to the chemical theory. In 1743, the Academy of Sciences of Bourdeaux proposed the ascent of vapours as the subject of a

[s] Optics, Qu. 31.

prize; which was adjudged in a manner very impartial as to the choice of a theory; for it was divided between Kratzenstein, who advocated the bubbles, (the coat of which he determined to be 1-50,000th of an inch thick,) and Hamberger, who maintained the truth to be the adhesion of particles of water to those of air and fire. The latter doctrine had become much more distinct in the author's mind when seven years afterwards (1750) he published his *Elementa Physices*. He then gave the explanation of evaporation in a phrase which has since been adopted,—the *solution of water in air;* which he conceived to be of the same kind as other chemical solutions.

This theory of solution was further advocated and developed by Le Roi[4]; and in his hands assumed a form which has been extensively adopted up to our times, and has, in many instances, tinged the language commonly used. He conceived that air, like other solvents, might be *saturated;* and that when the water was beyond the amount required for saturation, it appeared in a visible form. The saturating quantity was held to depend mainly on warmth and wind.

This theory was by no means devoid of merit; for it brought together many of the phenomena, and explained a number of the experiments which Le Roi made. It explained the facts of the transparency of vapour, (for perfect solutions are transparent,) the

[4] Ac. R. Sc. Paris, 1750.

precipitation of water by cooling, the disappearance of the visible moisture by warming it again, the increased evaporation by rain and wind; and other observed phenomena. So far, therefore, the introduction of the notion of the chemical solution of water in air was apparently very successful. But its defects are of a very fatal kind; for it does not at all apply to the facts which take place when air is excluded.

In Sweden, in the mean time[b], the subject had been pursued in a different, and in a more correct manner. Wallerius Ericsen had, by various experiments, established the important fact, that water evaporates in a vacuum. His experiments are clear and satisfactory; and he inferred from them the falsity of the common explanation of evaporation by the solution of water in *air*. His conclusions are drawn in a very intelligent manner. He considers the question whether water can be changed into air, and whether the atmosphere is, in consequence, a mere collection of vapours; and on good reasons, decides in the negative, and concludes the existence of permanently-elastic air different from vapour. He judges, also, that there are two causes concerned, one acting to produce the first ascent of vapours, the other to support them afterwards. The first, which acts in a vacuum, he conceives to be the mutual repulsion of the particles; and since this force is independent of the presence of other substances, this

[b] Fischer, Gesch. Phys. vol. v. p. 63.

seems to be a sound induction. When the vapours have once ascended into the air, it may readily be granted that they are carried higher, and driven from side to side by the currents of the atmosphere. Wallerius conceives that the vapour will rise till it gets into air of the same density as itself, and being then in equilibrium, will drift to and fro.

The two rival theories of evaporation, that of *chemical solution* and that of *independent vapour*, were, in various forms, advocated by the next generation of philosophers. Saussure may be considered as the leader on one side, and De Luc on the other. The former maintained the solution theory, with some modifications of his own. De Luc denied all solution, and held vapour to be a combination of the particles of water with fire, by which they became lighter than air. According to him, there is always fire enough present to produce this combination, so that evaporation goes on at all temperatures.

This mode of considering independent vapour as a combination of fire with water, led the attention of those who adopted the opinion to the thermometrical changes which take place when vapour is formed and condensed. These changes are important, and their laws curious. They belong to the induction of latent heat, of which we have just spoken; but they are not absolutely necessary in order to enable us to understand the manner in which steam exists in air.

De Luc's views led him[e] also to the consideration

[e] Fischer, vol. vii. p. 453. Nouvelles Idées sur le Météorologie, 1787.

of the effect of pressure on vapour. He explains the fact that pressure will condense vapour, by supposing that it brings the particles within the distance at which the repulsion arising from fire ceases. In this way, he also explains the fact, that though external pressure does thus condense steam, the mixture of a body of air, by which the pressure is equally increased, will not produce the same effect; and therefore, vapours can exist in the atmosphere. They make no fixed proportion of it; but at the same temperature we have the same pressure arising *from them*, whether they are in air or not. As the heat increases, vapour becomes capable of supporting a greater and greater pressure, and at the boiling heat, they can support the pressure of the atmosphere.

De Luc also marked very precisely (as Wallerius had done) the difference between vapour and air: the former being capable of change of consistence by cold or pressure, the latter not so. Pictet, in 1786, made a hygrometrical experiment, which appeared to him to confirm De Luc's views; and De Luc, in 1792, published a concluding essay on the subject in the Philosophical Transactions. Pictet's *Essay on Fire*, in 1791, also demonstrated that "all the train of hygrometrical phenomena takes place just as well, indeed rather quicker, in a vacuum than in air, provided the same quantity of moisture is present." This essay, and De Luc's paper, gave the deathblow to the theory of the solution of water in air.

Yet this theory did not fall without an obstinate

struggle. It was taken up by the new school of French chemists, and connected with their views of heat. Indeed it long appears as the prevalent opinion. Girtanner[7], in his *Grounds of the Antiphlogistic Theory*, may be considered as one of the principal expounders of this view of the matter. Hube, of Warsaw, was, however, the strongest of the defenders of the theory of solution, and published upon it repeatedly about 1790. He appears to have been somewhat embarrassed with the increase of the air's elasticity by vapour. Parrot, in 1801, proposed another theory, maintaining that De Luc had by no means successfully attacked that of solution, but only Saussure's superfluous additions to it.

It is difficult to see what prevented the general reception of the doctrine of independent vapour; since it explained all the facts very simply, and the agency of air was shown over and over again to be unnecessary. Yet, even now, the solution of water in air is hardly exploded. Gay-Lussac[8], in 1800, talks of the quantity of water "held in solution" by the air; which, he says, varies according to its temperature and density by a law which has not yet been discovered. And Professor Robison, in the article "Steam," in the *Encyclopædia Britannica* (published about 1800), says[9] "Many philosophers imagine that spontaneous evaporation, at low temperatures, is produced in this way, (by elasticity

[7] Fischer, vol. vii. 473. [8] Ann. Chim. tom. xliii.
[9] Robison's Works, ii. 37.

alone.) But we cannot be of this opinion; and must still think that this kind of evaporation is produced by the dissolving power of the air." He then gives some reasons for his opinion. "When moist air is suddenly rarefied, there is always a precipitation of water. But by this new doctrine the very contrary should happen, because the tendency of water to appear in the elastic form is promoted by removing the external pressure." Another main difficulty in the way of the doctrine of the mere mixture of vapour and air was supposed to be this; that if they were so mixed, the heavier fluid would take the lower, and the lighter the higher, part of the space which they occupied.

The former of these arguments was repelled by the consideration that in the rarefaction of air, its specific heat is changed, and thus its temperature reduced below the constituent temperature of the vapour which it contains. The latter argument is answered by a reference to Dalton's law of the mixture of gases. We must consider the establishment of these doctrines in a new section, as the most material step to the true notion of evaporation.

Sect. 2.—Dalton's Doctrine of Evaporation.

A PORTION of that which appears to be the true notion of evaporation was known, with greater or less distinctness, to several of the physical philosophers of whom we have spoken. They were aware that the vapour which exists in air, in an invisible

state, may be condensed into water by cold: and they had noticed that, in any state of the atmosphere, there is a certain temperature lower than that of the atmosphere, to which, if we depress bodies, water forms upon them in fine drops like dew; this temperature is hence called the *dew-point*. The water which exists anywhere may be reduced below the degree of heat which is necessary to constitute it vapour, and thus it ceases to be so. Hence this temperature is also called the *constituent temperature*. This was generally known to the meteorological speculators of the last century, although, in England, attention was principally called to it by Dr. Wells's *Essay on Dew*, in 1814. This doctrine readily explains how the cold produced by rarefaction of air, descending below the constituent temperature of the contained vapour, may precipitate a dew; and thus, as we have said, refutes one obvious objection to the theory of independent vapour.

The other difficulty was first fully removed by Mr. Dalton. When his attention was drawn to the subject of vapour, he saw insurmountable objections to the doctrine of a chemical union of water and air. In fact, this doctrine was a mere nominal explanation; for, on closer examination, no chemical analogies supported it. After some reflection, and in the sequel of other generalisations concerning gases, he was led to the persuasion, that when air and steam are mixed together, each follows its separate laws of equilibrium, the particles of each being elastic with [illegible] d to those of their own kind only: so that

steam may be conceived as flowing among the particles of air[10] "like a stream of water among pebbles;" and the resistance which air offers to evaporation arises, not from its weight, but from the inertia of its particles.

It will be found that the theory of independent vapour, understood with these conditions, will include all the facts of the case;—gradual evaporation in air; sudden evaporation in a vacuum; the increase of the air's elasticity by vapour; condensation by its various causes; and other phenomena.

But Mr. Dalton made experiments to prove his fundamental principle, that if two different gases communicate, they will diffuse themselves through each other[11];—slowly, if the opening of communication be small. He observes also, that all the gases had equal solvent powers, which could hardly have happened, had chemical affinity been concerned. Nor does the density of the air make any difference.

Taking all these circumstances into the account, Mr. Dalton abandoned the idea of solution. "In the autumn of 1801," he says, "I hit upon an idea which seemed to be exactly calculated to explain the phenomena of vapour: it gave rise to a great variety of experiments," which ended in fixing it in his mind as a true idea. "But," he adds, "the theory was almost universally misunderstood, and consequently reprobated."

Mr. Dalton answers various objections. Berthol-

[10] Manchester Memoirs, vol. v. p. 581.

[11] New System of Chemical Philosophy, vol. i. p. 151.

let had urged that we can hardly conceive the particles of a substance added to those of another, without increasing its elasticity. To this Mr. Dalton replies by adducing the instance of magnets, which repel each other, but not other bodies. One of the most curious and ingenious objections is that of Mr. Gough, who argues, that if each gas is elastic with regard to itself alone, we should hear, produced by one stroke, four sounds; namely, *first*, the sound through aqueous vapour; *second*, the sound through azotic gas; *third*, the sound through oxygen gas; *fourth*, the sound through carbonic acid. Mr. Dalton's answer is, that the difference of time at which these sounds would come is very small; and that, in fact, we do hear sounds double and treble.

In his *New System of Chemical Philosophy*, Mr. Dalton considers the objections of his opponents with singular candour and impartiality. He there appears disposed to abandon that part of the theory which negatives the mutual repulsion of the particles of the two gases, and to attribute their diffusion through one another to the different size of the particles, which would, he thinks[18], produce the same effect.

In selecting, as of permanent importance, the really valuable part of this theory, we must endeavour to leave out all that is doubtful or unproved. I believe it will be found that in all theories hitherto promulgated, all assertions respecting the properties

[18] New System, p. 188.

of the particles of bodies, their sizes, distances, attractions, and the like, are insecure and superfluous. Passing over, then, such hypotheses, the inductions which remain are these;—that two gases which are in communication will, by the elasticity of each, diffuse themselves in one another, quickly or slowly: and —that the quantity of steam contained in a certain space of air is the same, whatever be the air, whatever be its density, and even if there be a vacuum. These propositions may be included together by saying, that one gas is mechanically mixed with another; and we cannot but assent to what Mr. Dalton says of the latter fact,—"this is certainly the touchstone of the mechanical and chemical theories." This *doctrine of the mechanical mixture of gases* appears to supply answers to all the difficulties opposed to it by Berthollet and others, as Mr. Dalton has shown[18]; and we may, therefore, accept it as well established.

This doctrine, along with the *principle of the constituent temperature of steam*, are applicable to a large series of meteorological and other consequences. But before considering the applications of theory to natural phenomena, which have been made, it will be proper to speak of researches which were carried on, in a great measure, in consequence of the use of steam in the arts: I mean the laws which connect its elastic force with its constituent temperature.

[18] New System of Chemistry, vol. i. p. 160, &c.

Sect. 3.—*Determination of the Laws of the Elastic Force of Steam.*

THE expansion of aqueous vapour at different temperatures is governed, like that of all other vapours, by the law of Dalton and Gay-Lussac, already mentioned; and from this, its elasticity, when its expansion is resisted, will be known by the law of Boyle and Mariotte; namely, by the rule that the pressure of airy fluids is as the condensation. But it is to be observed, that this process of calculation goes on the supposition that the steam is cut off from contact with water, so that no more steam can be generated; a case quite different from the common one, in which the steam is more abundant as the heat is greater. The examination of the force of vapour, under these latter circumstances, must be briefly noticed.

During the period of which we have been speaking, the progress of the investigation of the laws of aqueous vapour was much accelerated by the growing importance of the steam-engine, in which those laws operated in a practical form. James Watt, the main improver of that machine, was thus a great contributor to speculative knowledge, as well as to practical power. Many of his improvements depended on the laws which regulate the quantity of heat which goes to the formation or condensation of steam, and the observations which led to these improvements enter into the induction of latent heat. Measurements of the force of steam, at all temperatures, were made with the same view. Watt's attention

had been drawn to the steam-engine in 1759, by Robison, the former being then an instrument-maker, and the latter a student at the University of Glasgow[14]. In 1761 or 1762, he tried some experiments on the force of steam in a Papin's Digester[15]; and formed a sort of working model of a steam-engine, feeling already his vocation to develope the powers of that invention. His knowledge was at that time principally derived from Desaguliers and Belidor, but his own experiments added to it rapidly. In 1764 and 1765, he made a more systematical course of experiments, directed to ascertain the force of steam. He tried this force, however, only at temperatures above the boiling-point; and inferred it at lower degrees from the supposed continuity of the law thus obtained. His friend Robison, also, was soon after led by reading the account of some experiments of Lord Charles Cavendish, and some others of Mr. Nairne, to examine the same subject. He made out a table of the correspondence of the elasticity and the temperature of vapour, from thirty-two to two hundred and eighty degrees of Fahrenheit's thermometer[16]. The

[14] Robison's Works, vol. ii. p. 113.

[15] Denis Papin, who made many of Boyle's experiments for him, had discovered that if the vapour be prevented from rising, the water becomes hotter than the usual boiling-point; and had hence invented the instrument called *Papin's Digester*. It is described in his book, *La manière d'amolir les os et de faire cuire toutes sortes de viandes en fort peu de temps et à peu de frais*. Paris, 1682.

[16] These were afterwards published in the Encyclopædia Britannica; in the article "Steam," written by Robison.

thing here to be remarked, is the establishment of a law of the pressure of steam, down to the freezing-point of water. Zeidler of Basle, in 1769, and Achard of Berlin, in 1782, made similar experiments. The latter examined also the elasticity of the vapour of alcohol. Betancourt, in 1792, published his Memoir on the expansive force of vapours; and his tables were for some time considered the most exact. Prony in his *Architecture Hydraulique* (1796), established a mathematical formula[17], on the experiments of Betancourt, who began his researches in the belief that he was first in the field, although he afterwards found that he had been anticipated by Ziegler. Gren compared the experiments of Betancourt and De Luc with his own. He ascertained an important fact, that when water *boils*, the elasticity of the steam is equal to that of the atmosphere. Schmidt at Giessen endeavoured to improve the apparatus used by Betancourt; and Biker, of Rotterdam, in 1800, made new trials for the same purpose.

In 1801, Mr. Dalton communicated to the Philosophical Society of Manchester his investigations on this subject; observing truly, that though the forces at high temperatures are most important when steam is considered as a mechanical agent, the progress of philosophy is more immediately interested in accurate observations on the force at low temperatures. He also found that his series of elasticities

[17] Architecture Hydraulique, Seconde Partie, p. 163.

for equidistant temperatures resembled a *geometrical progression*, but with a ratio constantly diminishing. Dr. Ure, in 1818, published in the Philosophical Transactions of London, experiments of the same kind, valuable from the high temperatures at which they were made, and for the simplicity of his apparatus. But in drawing from them his induction as to the law of elasticity, he appears to have had an unaccountable notion that the law of the force of steam was adjusted with a particular regard to arithmetical convenience; and that the simplicity of the rule, when expressed in decimals, was an almost undeniable argument in its favour. The law thus obtained approached, like Dalton's, to a *geometrical progression*. Dr. Ure says, that a formula proposed by M. Biot gives an error of near nine inches out of seventy-five, at a temperature of 266 degrees. This is very conceivable, for if the formula be wrong at all, the geometrical progress rapidly inflames the error in the higher portions of the scale. The elasticity of steam, at high temperatures, has also been experimentally examined by Mr. Southern, of Soho, and Mr. Sharpe, of Manchester. Mr. Dalton has attempted to deduce certain general laws from Mr. Sharpe's experiments; but we can hardly yet consider these as portions of established scientific truth. And I have given the above history rather on account of the importance of the research to the useful arts, than because it has led up to any simple law having the character of a discovery. But it may be remarked that no one appears to have sought

the law by employing the expansion of air as the measure of heat: although this step, as we have seen, has introduced into other parts of thermotics a simplicity and symmetry unknown before.

Sect. 4.—*Consequences of the Doctrine of Evaporation. Explanation of Rain, Dew, and Clouds.*

The discoveries concerning the relations of heat and moisture which were made during the last century, were principally suggested by meteorological inquiries, and were applied to meteorology as fast as they rose. Still there remains, on many points of this subject, so much doubt and obscurity, that we cannot suppose the doctrines to have assumed their final form, and therefore are not called upon to trace their progress and connexion. The principles of atmology are pretty well understood; but the difficulty of observing the conditions under which they produce their effects in the atmosphere is so great, that the precise theory of most meteorological phenomena is still to be determined.

We have already considered the answers given to the question, According to what rules does transparent aqueous vapour resume its form of visible water? This question includes, not only the problems of rain and dew, but also of clouds; for clouds are not vapour, but water, vapour being always invisible. An opinion which attracted much notice in its time, was that of Hutton, who, in 1784, endeavoured to prove that if two masses of air saturated

with transparent vapour at different temperatures are mixed together, the precipitation of water in the form either of cloud or of drops will take place. The reason he assigned for the opinion was this; that the temperature of the mixture is a mean between the two temperatures, but that the force of the vapour in the mixture, which is the mean of the forces of the two component vapours, will be greater than that which corresponds to the mean temperature, since the force increases faster than the temperature[18]; and hence some part of the vapour will be precipitated. This doctrine, it will be seen, speaks of vapour as "saturating" air, and is therefore inconsistent with Dalton's principle.

Dew.—The principle of a "constituent temperature" of steam, and the explanation of the "dew-point," were known, as we have said, to the meteorologists of the last century; but we perceive how incomplete their knowledge was, by the very gradual manner in which the consequences of this principle were traced out. We have already noticed, as one of the books which most drew attention to the true doctrine, in this country at least, Wells's *Essay on Dew*, published in 1814. In this work the author gives an account of the progress of his opinions[19]: "I was led," he says, "in the autumn of 1784, by the event of a rude experiment, to think it probable that the formation of dew is attended with the production of cold." This was confirmed by the experiments of others; but some years after,

[18] Edin. Trans. vol. i. p 42. [19] Essay on Dew, p. 1.

"upon considering the subject more closely, I began to suspect that Mr. Wilson, Mr. Six, and myself, had all committed an error in regarding the cold which accompanies the dew, as an *effect* of the formation of the dew." He now considered it rather as the *cause:* and soon found that he was able to account for the circumstances of this formation, many of them curious and paradoxical, by supposing the bodies on which dew is deposited, to be cooled down, by radiation into the clear night-sky, to the proper temperature. The same principle will obviously explain the formation of mists over streams and lakes when the air is cooler than the water; which was put forward by Davy, even in 1819, as a new doctrine, or at least as not familiar.

Hygrometers.—According as air has more or less of vapour in comparison with that which its temperature and pressure enable it to contain, it is more or less humid; and an instrument which measures the degrees of such a gradation is a *hygrometer.* The hygrometers which were at first invented, were those which measured the moisture by its effect in producing expansion or contraction in certain organic substances; thus Saussure devised a hair-hygrometer, Deluc a whalebone-hygrometer, and Dalton used a piece of whipcord. All these contrivances were variable in the amount of their indications under the same circumstances; and, moreover, it was not easy to know the physical meaning of the degree indicated. The dew-point, or constituent temperature of the vapour which exists in the air, is, on the other hand, both

constant and definite. The determination of this point, as a datum for the moisture of the atmosphere, was employed by Le Roi, and by Dalton (1802), the condensation being obtained by cold water[20]: and finally, Mr. Daniell (1812) constructed an instrument, where the condensing temperature was produced by evaporation of ether, in a very convenient manner. This invention (*Daniell's Hygrometer*) enables us to determine the quantity of vapour which exists in a given mass of the atmosphere at any time of observation.

Clouds.—When vapour becomes visible by being cooled below its constituent temperature, it forms itself into a fine watery powder, the diameter of the particles of which this powder consists being very small: they are estimated by various writers, from 1-100,000th to 1-20,000th of an inch[21]. Such particles, even if solid, would descend very slowly; and very slight causes would suffice for their suspension, without recurring to the hypothesis of vesicles, of which we have already spoken. Indeed that hypothesis will not explain the fact, except we suppose these vesicles filled with a rarer air than that of the atmosphere; and, accordingly, though this hypothesis is still maintained by some[22], it is asserted as a fact of observation, proved by optical or other phenomena, and not deduced from the suspension of clouds. The latter result is still variously explained

[20] Daniell, Met. Ess. p. 142. Manch. Mem. vol. v. p. 581.
[21] Kæmtz. Met. i. 393. [22] Ib. i. 393. Robison, ii. 13.

by different philosophers: thus, Gay-Lussac[83] accounts for it by upward currents of air, and Fresnel explains it by the heat and rarefaction of air in the interior of the cloud.

Classification of Clouds.—A classification of clouds can then only be consistent and intelligible when it rests upon their atmological conditions. Such a system was proposed by Mr. Luke Howard, 1802-3. His primary modifications are, *Cirrus*, *Cumulus*, and *Stratus*, which the Germans have translated by terms equivalent in English to *feather-cloud*, *heap-cloud*, and *layer-cloud*. The cumulus increases by accumulations on its top, and floats in the air with a horizontal base; the stratus grows from below, and spreads along the earth; the cirrus consists of fibres in the higher regions of the atmosphere, which grow every way. Between these simple modifications are intermediate ones, *cirro-cumulus* and *cirro-stratus;* and, again, compound ones, the *cumulo-stratus* and the *nimbus*, or *rain-cloud*. These distinctions have been generally accepted all over Europe: and have rendered a description of the processes which go on in the atmosphere far more definite and clear than it could be made before their use.

I omit a vast mass of facts and opinions, supposed laws of phenomena and assigned causes, which abound in meteorology more than in any other science. The slightest consideration will show us what a vast amount of labour, of persevering and combined

[83] Ann. Chim. xxv. 1822.

observation, the progress of this branch of knowledge requires. I do not even speak of the condition of the more elevated parts of the atmosphere. The diminution of temperature as we ascend, one of the most marked of atmospheric facts, has been variously explained by different writers. Thus Dalton[24] (1808) refers it to a principle "that each atom of air, in the same perpendicular column, is possessed of the same degree of heat," which principle he conceives to be entirely empirical in this case. Fourier says[25] (1817) "This phenomenon results from several causes: one of the principal is the progressive extinction of the rays of heat in the successive strata of the atmosphere."

Leaving, therefore, the application of thermotical and atmological principles in particular cases, let us consider for a moment the general views to which they have led philosophers.

[24] New Syst. of Chem. vol. i. p. 125. [25] Ann. Chim. vi. 286.

CHAPTER IV.

Physical Theories of Heat.

When we look at the condition of that branch of knowledge which, according to the phraseology already employed, we must call *Physical Thermotics*, in opposition to Formal Thermotics, which gives us detached laws of phenomena, we find the prospect very different from that which was presented to us by physical astronomy, optics, and acoustics. In these sciences, the maintainers of a distinct and comprehensive theory have professed at least to show that it explains and includes the principal laws of phenomena of very various kinds: in thermotics, we have only attempts to explain a part of the facts. We have here no example of an hypothesis which, assumed in order to explain one class of phenomena, has been found also to account exactly for another; as when central forces led to the precession of the equinoxes; or when the explanation of polarization explained also double refraction; or when the pressure of the atmosphere, as measured by the barometer, gave the true velocity of sound. Such coincidences are the test of truth; and thermotical theories cannot yet exhibit credentials of this kind.

On looking back at our view of this science, it will be seen that it may be distinguished into two parts; the Doctrines of Conduction and Radiation,

which we call Thermotics proper; and the Doctrines respecting the relation of Heat, Airs, and Moisture, which we have termed Atmology. These two subjects differ in their bearing on our hypothetical views.

Thermotical Theories.—The phenomena of radiant heat, like those of radiant light, obviously admit of general explanation in two different ways;—by the emission of material particles, or by the propagation of undulations. Both these opinions have found supporters. Probably most persons, in adopting Prevost's theory of exchanges, conceive the radiation of heat to be the radiation of matter. The undulation hypothesis, on the other hand, appears to be suggested by the production of heat by friction, and was accordingly maintained by Rumford and others. Leslie[1] appears, in a great part of his "Inquiry," to be a supporter of some undulatory doctrine, but it is extremely difficult to make out what his undulating medium is; or rather, his opinions wavered during his progress. In page 31, he asks, "What is this calorific and frigorific fluid?" and after keeping the reader in suspense for a moment, he replies,

"Quod petis hic est.

It is merely the ambient AIR." But at page 150, he again asks the question, and, at page 188, he answers, "It is the same subtile matter that, accord-

[1] An Experimental Inquiry into the Nature and Propagation of Heat. 1804.

ing to its different modes of existence, constitutes either heat or light." A person thus vacillating between two opinions, one of which is palpably false, and the other laden with exceeding difficulties which he does not even attempt to remove, had little right to protest against[a] "the sportive freaks of some intangible *aura*;" to rank all other hypotheses than his own with the "occult qualities of the schools;" and to class the "prejudices" of his opponents with the tenets of those who maintained the *fuga vacui* in opposition to Torricelli. It is worth while noticing this kind of rhetoric, in order to observe, that it may be used just as easily on the wrong side as on the right.

Till recently, the theory of material heat, and of its propagation by emission, was probably the one most in favour with those who had studied mathematical thermotics. As we have said, the laws of conduction, in their ultimate analytical form, were almost identical with the laws of motion of fluids. Fourier's principle also, that the radiation of heat takes place from points below the surface, and is intercepted by the superficial particles, appears to favour the notion of material emission.

Accordingly, some of the most eminent modern French mathematicians have accepted and extended the hypothesis of a material caloric. In addition to Fourier's doctrine of molecular extra-radiation, Laplace and Poisson have maintained the hypothesis of *molecular intra-radiation*, as the mode in which

[a] Inquiry, p. 47.

conduction takes place: that is, they say that the particles of bodies are to be considered as *discrete*, or as points separated from each other, and acting on each other at a distance; and the conduction of heat from one part to another, is performed by radiation between all neighbouring particles. They hold that, without this hypothesis, the differential equations expressing the conditions of conduction cannot be made homogeneous: but this assertion rests, I conceive, on an error, as Fourier has shown, by dispensing with it. The necessity of the hypothesis of discrete molecular action in bodies, is maintained in all cases by M. Poisson; and he has asserted Laplace's theory of capillary attraction to be defective on this ground, as Laplace asserted Fourier's reasoning respecting heat to be so. In reality, however, this hypothesis of discrete molecules cannot be maintained as a physical truth; for the law of molecular action, which is assumed in the reasoning, after answering its purpose in the progress of calculation, vanishes in the result; the conclusion is the same, whatever law of the intervals of the molecules be assumed. The definite integral, which expresses the whole action, no more proves that this action is made up of the differential parts by means of which it was found, than the processes of finding the weight of a body by integration, prove it to be made up of differential weights. And therefore, even if we were to adopt the emission theory of heat, we are by no means bound to take along with it the hypothesis of discrete molecules.

But the recent discovery of the refraction, polarization, and depolarization of heat has quite altered the theoretical aspect of the subject, and, almost at a single blow, ruined the emission theory. Since heat is reflected and refracted like light, analogy would lead us to conclude that the mechanism of the processes is the same in the two cases. And when we add to these the property of polarization, it is scarcely possible to believe otherwise than that heat consists in transverse vibrations; for no wise philosopher would attempt an explanation by ascribing poles to the emitted particles, after the experience which optics affords, of the utter failure of such machinery.

But here the question occurs, If heat consist in vibrations, whence arises the extraordinary identity of the laws of its propagation with the laws of the flow of matter? How is it that, in conducted heat, this vibration creeps slowly from one part of the body to another, the part first heated remaining hottest; instead of leaving its first place and travelling rapidly to another, as the vibrations of sound and light do? The answer to these questions has been put in a very distinct and plausible form by that distinguished philosopher, M. Ampère, who published[3] a *Note on Heat and Light considered as the results of Vibratory Motion*, in 1834 and 1835; and though this answer is an hypothesis, it at least shows that there is no force in the difficulty.

M. Ampère's hypothesis is this; that bodies con-

[3] Bibliotheque Universelle de Geneve, vol. xlix. p. 225. Ann. Chim. tom. lviii. p. 434.

sist of solid molecules, which may be considered as arranged at intervals in a very rare ether; and that the vibrations of the molecules, causing vibrations of the ether and caused by them, constitute heat. On these suppositions, we should have the phenomena of conduction explained; for if the molecules at one end of a bar be hot, and therefore in a state of vibration, while the others are at rest, the vibrating molecules propagate vibrations in the ether, but these do not produce heat, except in proportion as they put the quiescent molecules of the bar in vibration; and the ether being very rare compared with the molecules, it is only by the repeated impulses of many successive vibrations that the nearest quiescent molecules are made to vibrate, after which they combine in communicating the vibration to the more remote molecules. "We then find necessarily," M. Ampère adds, "the same equations as those found by Fourier for the distribution of heat, setting out from the same hypothesis, that the temperature or heat transmitted is proportional to the difference of the temperatures."

Since the undulatory hypothesis of heat can thus answer all obvious objections, we may consider it as upon its trial, to be confirmed or modified by future discoveries, and especially by an enlarged knowledge of the laws of the polarization of heat.

Atmological Theories.—Hypotheses of the relations of heat and air almost necessarily involve a reference to the forces by which the composition of bodies is produced, and thus cannot properly be

treated of till we have surveyed the condition of chemical knowledge. But we may say a few words on one such hypothesis, I mean the hypothesis on the subject of the atmological laws of heat, proposed by Laplace, in the twelfth book of the "Mécanique Céleste," and published in 1823. It will be recollected that the main laws of phenomena for which we have to account, by means of such an hypothesis, are the following:—

1. The law of Boyle and Mariotte, that the elasticity of an air is as the density.

2. The law of Gay-Lussac and Dalton, that all airs expand equally by heat.

3. The increase of heat by compression of airs.

4. Dalton's principle of the mechanical mixture of airs.

5. Expansion by heat in solids and fluids.

6. The effect of heat in causing change of consistence, and the doctrine of latent heat.

Besides these, there are laws of which it is doubtful whether they are or are not included in the preceding, as the low temperature of the air in the higher parts of the atmosphere.

Laplace's hypothesis[4] is this:—that bodies consist of particles, each of which gathers round it, by its attraction, a quantity of caloric: that the particles of the bodies attract each other, besides attracting the caloric, and that the particles of the caloric repel each other. In gases, the particles of the bodies are

[4] Méc. Cél. t. v. p. 89.

so far removed, that their mutual attraction is insensible, and the matter tends to expand by the mutual repulsion of the caloric. He conceives this caloric to be constantly radiating among the particles; the density of this internal radiation is the *temperature*, and he proves that, on this supposition, the elasticity of the air will be as the density, and as this temperature. Hence follow the three first rules above stated. The same suppositions lead to Dalton's principle of mixtures, though without involving his mode of conception; for Laplace says that whatever the mutual action of two gases be, the whole pressure will be equal to the sum of the separate pressures[5]. Expansion, and the changes of consistence, are explained by supposing[6] that in solids, the mutual attraction of the particles of the body is the greatest force, in liquids, the attraction of the particles for the caloric, in airs, the repulsion of the caloric. But the doctrine of latent heat again modifies[7] the hypothesis, and makes it necessary to include latent heat in the calculation; yet there is not, as we might hope there would be if the theory were the true one, any confirmation of the hypothesis resulting from the new class of laws thus referred to.

It will be observed that Laplace's hypothesis goes entirely upon the materiality of heat, and is inconsistent with any vibratory theory; for, as Ampère remarks, "It is clear that if we admit heat to consist in vibrations, it is a contradiction to attribute to

[5] Méc. Cél. t. v. p. 110. [6] Ib. p. 92. [7] Ib. p. 93.

heat (or caloric) a repulsive force of the particles which would be a cause of vibration."

An unfavourable judgment of Laplace's theory of gases is suggested by looking for that which, in speaking of optics, was mentioned as the great characteristic of a true theory; namely, that the hypotheses, which were assumed in order to account for one class of facts, are found to explain another class of a different nature. Thus, in thermotics, the law of an intensity of radiation proportional to the sine of the angle of the ray with the surface, which is founded on direct experiments of radiation, is found to be necessary in order to explain the tendency of neighbouring bodies to equality of temperature; and this leads to the higher generalisation, that heat is radiant from below the surface. But in the doctrine of the relation of heat to gases, as delivered by Laplace, there is none of this unexpected confirmation; and though he explains some of the leading laws, his assumptions bear a large proportion to the laws explained. Thus, from the assumption that the repulsion of gases arises from the mutual repulsion of the particles of caloric, he finds that the pressure in any gas is as the square of the density and of the quantity of caloric[8]; and from the assumption that the temperature is the internal radiation, he finds that this temperature is as the density and the square of the caloric[9]. Hence he obtains the law of Boyle and Mariotte, and that of Dalton and Gay-Lussac.

[8] $P = 2\pi n k \rho^2 c^2$ (1) p. 107. [9] $q' \Pi(a) = \rho c^2$; (2) p. 108.

But this view of the subject requires other assumptions when we come to latent heat; and accordingly, he introduces, to express the latent heat, a new quantity[10]. Yet this quantity produces no effect on his calculations, nor does he apply his reasoning to any problem in which latent heat is concerned.

Without, then, deciding upon this theory, we may venture to say that it is wanting in all the prominent and striking characteristics which we have found in those great theories which we look upon as clearly and indisputably established.

Conclusion.—We may observe, moreover, that heat has other bearings and effects, which, as soon as they have been analyzed into numerical laws of phenomena, must be attended to in the formation of thermotical theories. Chemistry will probably supply many such; those which occur to us we must examine hereafter. But we may mention as examples of such, De la Rive and Marcet's law, that the specific heat of all gases is the same[11]; and Dulong and Petit's law, that single atoms of all simple bodies have the same capacity for heat[12]. Though we have not yet said anything of the relation of different gases, or explained the meaning of atoms in the chemical sense, it will easily be conceived that these are very general and important propositions.

Thus the science of thermotics, imperfect as it is, forms a highly-instructive part of our survey, and is

[10] The quantity *i*, p. 113. [11] Ann. Chim. xxxv. (1827.)
[12] Ib. x. 397.

one of the cardinal points on which the doors of those chambers of physical knowledge must turn which hitherto have remained closed. For, on the one hand, this science is related by strong analogies and dependencies to the most complete portions of our knowledge, our mechanical principles and optical theories; and on the other, it is connected with properties and laws of a nature altogether different,—those of chemistry; properties and laws depending upon a new system of notions and relations, among which clear and substantial general principles are far more difficult to lay hold of, and with which the future progress of human knowledge appears to be far more concerned. To these notions and relations we must now proceed; but we shall find an intermediate stage, in certain subjects which I shall call the *mechanico-chemical* sciences; namely, those which have to do with magnetism, electricity, and galvanism.

END OF THE SECOND VOLUME.

ERRATA IN VOL. II.

Page 103, line 14, *for* geometry, *read* geometers.
107, line 24, *for* two, *read* three.
369, line 13, *for* correct, *read* connect.

Insert the following explanation of technical terms which are introduced without sufficient notice of their meaning.

Page 375, line 4. *The Plane of Polarization.*

375, line last, *read* plane of polarization being perpendicular to the plane of refraction;

Each image of a ray of polarized light, seen through a transparent crystal of Iceland spar, varies in intensity in different positions of the crystal, as stated in page 374; and if the ray be completely polarized, each image is extinguished in a certain position of the principal plane of the crystal, and is brightest in a position of the plane perpendicular to this; the ray is extinguished also by various other reflecting and refracting apparatus, (which would not extinguish common light,) but always in a particular position of some principal plane of the apparatus, and is always brightest in the perpendicular position of the plane. The ray is said to be polarized *in the plane*, which thus gives the brightest effect from it.

The term *polarized* was employed to describe this modification of light, because it was represented to the mind by conceiving that the particles of the light had *poles*, and that they were propagated or stopped, accordingly as the poles were in the plane of polarization or perpendicular to it.

But even when we reject this mode of conceiving the property, the term *polarized* is still suitable and philosophical; for in all cases we may rightly call those properties *polar* which exhibit *opposite results* in *opposite positions*.

Page 384, line 10, (*Polar*) *Analysis of Light.*

The prism of Iceland spar here spoken of, produces colours by separating the transmitted ray according to the laws of double refraction; hence it is said to *analyse* the light.

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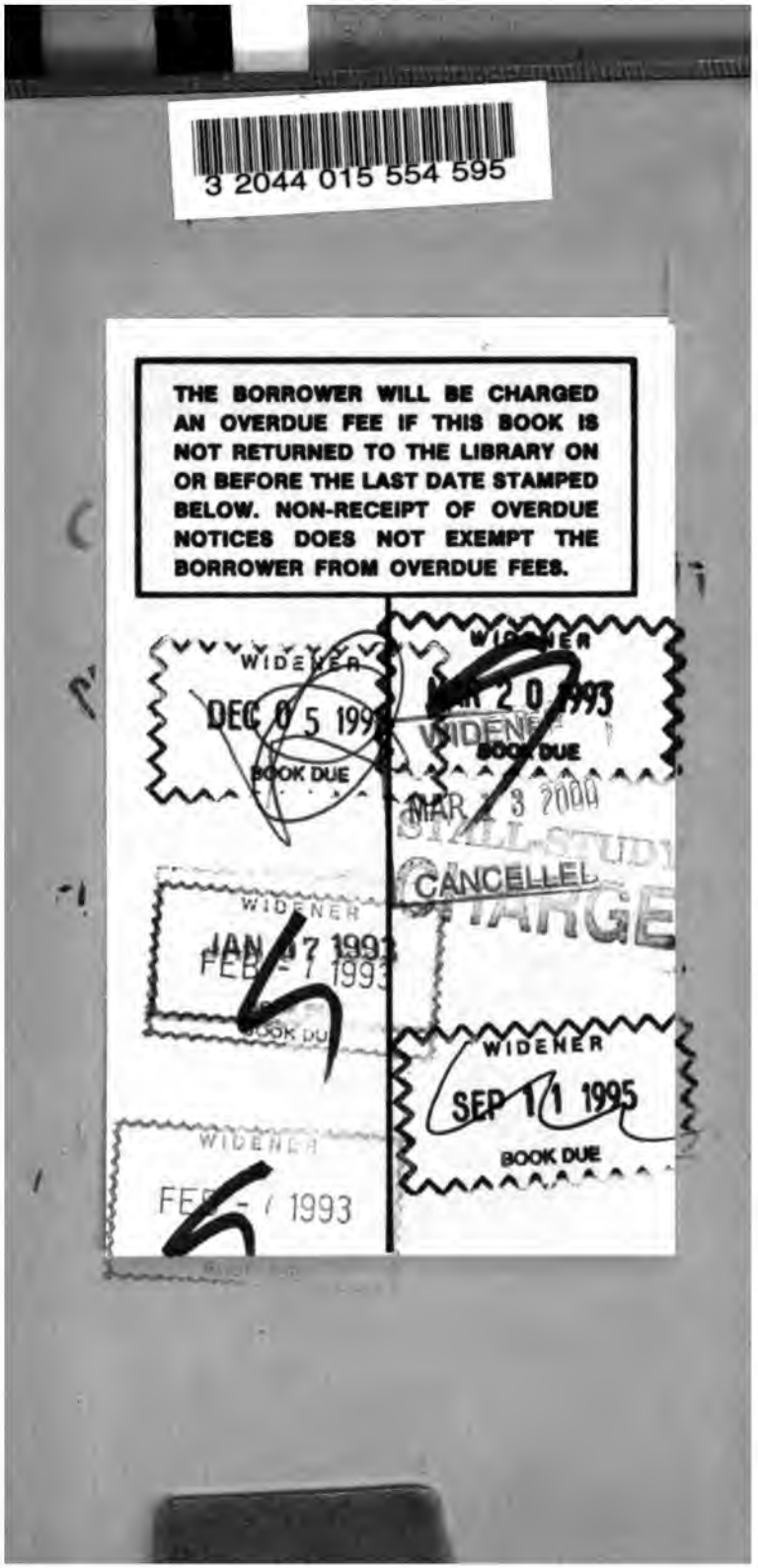
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